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VIVISECTIONS

AND

PAINFUL EXPERIMENTS ON LIVING ANIMALS:

THEIR UNJUSTIFIABILITY.

BY

W. GIMSON GIMSON, M.D., M.R.C.S.,

WITHAM, ESSEX.

"'Tis Nature's law
That none, the meanest of created things,
Of forms created the most vile and brute,
The dullest or most noxious, should exist
Divorced from good—a spirit and pulse of good,
A life and soul, to every mode of being
Inseparably linked."

WORDSWORTH.

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INTRODUCTORY.

Have vivisections and painful experiments been of any scientific value?

Have they led to the discovery of scientific facts of permanent importance?

Are there not fallacies underlying such a method of interrogating Nature, which of necessity vitiate the results?

In the following pages I shall endeavour to produce arguments which to me appear best to meet these several propositions. I shall draw largely for my evidence from books which are considered standard works upon physiology, anatomy, and pathology, and also from the lectures and writings of those experimental physiologists who have worked so zealously in what they have considered the high-road to physiological knowledge. And I hope to prove, from the writings of the latter, that *painful experiments* upon living animals have tended rather to obscure and

retard the progress of scientific knowledge, than to elucidate and hasten the advance of physiological truths. Experiments to watch the natural functions of the body in animals elicit our warmest approbation; these *must* be prosecuted under circumstances most in accordance with the habits of the animal, and so tend to its comfort. And it may strike some as singular, but it is nevertheless a fact, that, so long as physiological inquiry proceeds on this ground, its advance is satisfactory in results; but no sooner do we interfere with the chain of actions going on in the body, than we produce phenomena which are confusing and unsatisfactory. If any one is sceptical on this point, let him read carefully any standard author of physiology, and let him classify the conclusions he finds demonstrated, and note the results; he will find my assertion borne out, unless I have erred in my reckoning.

And if in my progress I quote from any author without due acknowledgment, it will, I hope, be assumed to be from inadvertence,

and not from misappropriation. My desire and earnest wish will be to paint, in the clearest colours, the various views as they present themselves to my mind, with the hope that many will be induced to look upon *painful* experimentation as unwarrantable.

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VIVISECTIONS

AND PAINFUL EXPERIMENTS ON LIVING ANIMALS.

THE discovery of the circulation of the blood, so often quoted as a result of experiments upon living animals, may form a fitting subject wherewith to commence the present essay.

Although the ancients are supposed to have been ignorant of the circulation of the blood, physicians of the remotest antiquity seem to have entertained the idea, that in man and the higher animals, the blood is contained in a system of vessels connected with the heart.

Galen was the first to prove that the arteries contained blood during life (De Anatom. Admin. vii. 5) contrary to the prevailing error of believing that they contained not blood, but air. He also asserted the existence of a pul-

monary circulation; but fell into error when he supposed that the greater portion of the blood passed directly from the right to the left side of the heart through numerous channels existing in the septum which intervenes between the ventricles.

The opinions of Galen were accepted until the commencement of the fourteenth century, but an important advance was made in the early part of the sixteenth century by Berenger of Carpi, who pointed out the error into which Galen had fallen with reference to the channels existing in the interventricular septum. Berenger's opinion was upheld by Mich. Servetus, a Spanish physician, who, about the year 1552, alleged that the septum is really solid, and thence inferred that the blood in its transit from the right to the left side of the heart passes through the lungs; he wrote,—"sanguine, quem dexter ventriculus cordis sinistro communicat. Fit autem communicatio hæc, non per parietem cordis medium, ut vulgo creditur, sed magno artificio, a dextro cordis ventriculo, longo per

pulmones ductu, agitatur sanguis subtilis; et a vena arteriosa, in arteriam venosam transfunditur. Deinde, in ipsa arteria venosa inspirato aeri miscetur, expiratione a fuligine repurgatur. Atque ita tandem a *sinistro cordis ventriculo totum mixtum per diastolem attrahitur*, apta suppellex, ut fiat spiritus vitalis. Quod ita per pulmones fiat communicatio et præparatio, docet conjunctio vario, et communicatio venæ arteriosæ cum arteria venosa in pulmonibus. Confirmat hæc magnitudo insignis venæ arteriosæ, quæ nec talis, nec tanta facta esset, nec tantam a corde ipso vim purissimi sanguinis in pulmones emitteret ob solum eorum nutrimentum, &c. Item, a pulmonibus ad cor non simplex aer, sed mixtus sanguine mittitur per arteriam venosam; ergo, in pulmonibus, fit mixtio, &c. Illa itaque spiritus vitalis, a sinistro cordis ventriculo, in arterias totius corporis deinde transfunditur, &c." (Elliotson, p. 195.)

Cesalpino of all investigators prior to Harvey arrived nearest to the discovery of a systemic circulation. He wrote about 1569—

1580: "Of the vessels terminating in the heart, some introduce their contents into its substance, as the vena cava (arteria venalis) in the right chamber, and the pulmonary veins (vena arterialis) in the left;' others convey it away, as the aortal artery from the left chamber, and the pulmonary artery from the right. All are provided with membranes, so fitted to their office, that the admitting valves can never *educe*, nor the educing valves admit; it follows that the heart contracting, the arteries dilate, and it dilating they contract: not simultaneously as it seems at first. . . . . Nor is there any danger of regurgitation from the arteries into the heart. For a motion takes place from the veins into the heart, which, by its heat attracts nourishment to itself; at the same time, also from the heart into the arteries, this being the only road open on account of the position of the membranes. Thus the same motion opens both kinds of valves—that is, of the veins into the heart, and of the heart into the arteries — the membranes being at the same time so

arranged, that the contrary motion can never occur." (Quæstion. Peripat., lib. v., quæst. 4, quoted in Edinburgh Review, No. 301.) Cesalpino knew well the circulation through the lungs from dissections: and observed the swelling of a vein on the side of a ligature away from the heart, from which he argued, that the opinion of a centrifugal movement of the blood in the veins was wrong: although, ignorant of the existence of valves in the veins, he speaks of a flux and reflux of the blood in the veins.

Fabricius ab Acquapendente amongst others, discovered and described the valves in the veins.

For our countryman Harvey was reserved the honour of making the discovery of the circulation of the blood, which was promulgated by him in an anatomical and surgical course of lectures at the College of Physicians in 1619, and which is associated with his name in the present day.

"He is entitled to the glory of having made it," says Hume (Hist. of England,

ch. 62), "by reasoning alone, without any mixture of accident." He informed Boyle, that he was led to it, by reflecting on the arrangement of the valves of the heart and veins, as exhibited by his master Fabricius. Nothing, he knew, was planned in vain, and these clearly allowed a fluid to pass but one way. By this argument, and the fact of a ligature upon an *artery* causing the blood to accumulate in it, on the side nearest the heart, and upon a *vein*, beyond the ligature;* and that animals bleed to death by wounds in arteries and veins, he chiefly established his doctrine. His immediate reward was general ridicule and abuse, and a great diminution of his practice; and no physician in Europe, who at the time had reached forty years of age, ever, to the end of life, adopted his doctrine of the circulation." (Hume.)

"The singular structure of the parts concerned," writes Dr. Wm. Hunter, 1784, "so evidently proclaims the circulation, that there

* *Vide* Harvey's Works, by Dr. Willis, pp. 56, *et seq.* Ligature applied not to vessel but to limb.

seems to have been nothing more required for making the discovery, than laying aside gross prejudices, and considering fairly, some obvious truths. Servetus first, and Columbus afterwards, both in the time of Vesalius, had clearly given the circulation of the blood through the lungs, which we may reckon at least three-fourths of the discovery; and Cesalpinus had many years before Harvey, published in three different works all that was wanting in Servetus to make the circulation quite complete." (Edinburgh Review, No. 301.)

Harvey's doctrine gave rise to much opposition, and, writes Haller (El. Physiology, t. i. p. 243), "when the truth could be denied no longer he was pronounced a plagiarist: the circulation was declared to have been known to Plato; nay, more, to King Solomon."

Great, however, as the doctrine promulgated by Harvey was, a connecting link was wanting to make the circulation complete. Nor was this supplied until four years after the death of Harvey, when Malpighi in 1661, demonstrated by the aid of the microscope,

the passage of blood through the smallest bloodvessels, and so established the reality of the communication between the arteries and the veins. To demonstrate this communication, no painful experiment was necessary, and we are told, that, "among warm-blooded animals, the egg, especially at the fourth and fifth day of incubation, if placed under a simple microscope, is most adapted for the demonstration of the circulation." (Elliotson Physiol., note, p. 162.)

We have endeavoured to give an account of what we have gleaned from various authors concerning the discovery of the circulation of the blood, and since many, as stated in one of our leading journals of the day, still claim Harvey's discovery as the result of "numerous vivisections," we will give the English of Harvey's own words upon this subject.

"*Whether* derived from vivisections, and my various reflections on them, *or* from the ventricles of the heart and the vessels that enter into and issue from them, the symmetry and size of these conduits,—for nature doing

nothing in vain, would never have given them so large a size without a purpose,— *or* from the arrangement and intimate structure of the valves in particular, *and* of the other parts of the heart in general, with many things besides, I frequently and seriously bethought me, and long revolved in my mind, what might be the quantity of blood which was transmitted, in how short a time its passage might be effected, and the like; and not finding it possible that this could be supplied by the juices of the ingested aliment without the veins on the one hand becoming drained, and the arteries on the other getting ruptured through the excessive charge of blood, unless the blood should somehow find its way from the arteries to the veins, and so return to the right side of the heart; I began to think whether there might not be *a motion, as it were, in a circle.*"

"That the pulses of arteries are due to the impulses of the blood from the left ventricle, I happened upon one occasion to have a particular case under my care, which plainly

satisfied me of this truth: A certain person was affected with a large pulsating tumour on the right side of the neck, called an aneurism, . . . this tumour was visibly distended as it received the charge of blood brought to it by the artery, with each stroke of the heart: the connection of parts was obvious when the body of the patient came to be opened *after* his death."

Again we find Harvey using very guarded language, when writing to one of his critics,* he says, "But if you will kindly refer to my eighth and ninth chapters, you will find it stated in so many words that I have purposely omitted to speak of . . . the causes of this motion and circulation, especially of the final cause."

These quotations surely do not bear out the assertion recently made that the circulation was discovered by "numerous vivisections," unless, living two hundred and fifty years after Harvey, we are more able to judge of his process of reasoning than he was himself.

* Caspar Hoffman, Letter dated May 20, 1636.

That Harvey frequently had recourse to vivisections, and that he employed a variety of animals for his experiments we learn from his writings—but that he relied upon anatomy and logical reasoning to prove his doctrine of the circulation of the blood, is apparent to any one who will read his works as translated by Dr. Willis. No amount of vivisections could have demonstrated in Harvey's days the results he obtained, for we must remember that the microscope was then unknown, that what was revealed four years after his death by means of that instrument, was incorporated into a doctrine by Harvey as the result of reasoning. *No amount* of vivisections *without* the use of the microscope could have determined the communications existing between the arteries and the veins, the one link which was missing in Harvey's chain; and which *with* the aid of the microscope can be proved and demonstrated *without* a single painful experiment upon any animal.

And writing two hundred and fifty years after Harvey wrote, in the guarded language

already quoted, De Motu Cordis, we are compelled to own that "None of these theories, however, appear to afford a sufficient explanation of all the facts, and the essential cause of the rhythmical action of the heart must still remain an unsolved question." (Carpenter.)

Rather than seeking by experiments to discover these secrets, let us be, not *almost* (to use Harvey's words), but *quite* tempted to think, "that the motion of the heart was only to be comprehended by God."

Long before the discovery of Harvey, the diseases and injuries of the circulating system had engaged the attention of men of science. Hæmorrhage had from time out of mind been a cause of terror and dismay to mankind, and for a long time the fear of it had retarded the improvement of surgery. Morand well describes the feeling of apprehension which pervaded the minds not only of the public, but of the old surgeons, when he writes:—

"Un sentiment naturel attache à l'idée de perdre son sang; une terreur machinale, dont l'enfant qui commence à parler, et l'homme le

plus décidé sont également susceptibles. On ne peut point dire, que cette peur soit chimérique. Si l'on comptoit ceux qui perdent la vie dans une bataille, on verroit que les trois quarts ont péri par quelque hémorrhagie; et dans les grandes opérations de chirurgie cet accident est presque toujours le plus formidable."

The fear of hæmorrhage, and the co-existing ignorance of how to stop the bleeding, made the older surgeons hesitate to use the knife, lest their patient should bleed to death. The actual cautery, caustics, astringents, and various means were used to arrest or stay the flow of blood, but to those persons who claim the use of the ligature for that purpose, for the discovery of Harvey, we commend the following. Celsus, when the hæmorrhage resisted the then ordinary methods, advised *two ligatures to be applied to the wounded part of the vessel, and then dividing the portion situated between them;* —"Quod si illa quoque profluvio vincuntur, venæ, quæ sanguinem fundunt, apprehendendæ, circaque id, quod ictum est, duobus locis

deligandæ, intercidendæque sunt, ut et in se ipsæ coeant, et nihilominus ora præclusa habeant." Lib. 5, cap. 26.

Galen also mentions tying vessels for the purpose of stopping hæmorrhage, and there are some traces of the same information in other authors who lived before him.

Ambrose Paré is considered as the first who regularly employed the ligature after amputation. Paré is, however, very careful not to take to himself any credit for so doing. In his *Apologie* he takes great care to impute the origin of it to the ancients; and cites many of them, who had made mention of it. His own first adoption of the practice, he ascribes to inspiration of the Deity.

But, it is not to the application of the ligature as a means of arresting hæmorrhage, that we would draw undivided attention; rather would we point to the use of this agent in the treatment of the disease known under the name of aneurism, as leading to some of the boldest and most successful surgical operations at the present day.

An aneurism may be described as a tumour filled with blood, from the rupture, wound, ulceration, or simple dilatation of an artery: having a tendency to increase, and to ultimately cause death by hæmorrhage. Cases have been found in which nature has proved sufficiently powerful to effect a cure, but these form the exception. The great physiologist Haller had asserted, that he could imitate in animals the formation of an aneurism, and that he could readily produce one, by separating the fibrous coat from the inner coat of an artery.

John Hunter denied that this could be done; he laid bare the carotid artery (of a dog), afterwards "skinned it with a knife even to transparency," but no dilatation of the vessel took place: on the contrary, the vessel was rendered stronger, as Hunter declared it would be, in consequence of the adhesive inflammation taking place.

Haller, in his experiments, left the denuded vessel to remain isolated and separated from the adjoining soft parts: Hunter allowed the artery and the surrounding cellular tissue to

with a firm roller; and thus obtained the speedy obliteration of the vessel, and cured the aneurism, which had been first injudiciously laid open.

The application of the ligature for the cure of aneurism, viz. the cutting down upon the tumour, turning out the contents, and securing both ends of the vessel, yielded most unsatisfactory results; and it was not until John Hunter, in 1785, practised an operation the result of careful reasoning, that any real advance was made. Hunter believed that aneurismal arteries were usually diseased, and therefore he applied his ligature at a distance from the swelling, and did not open the tumour at all: his principles of adhesion and absorption, powers which he had learned to recognise, were the physiological resources on which he relied, for the gradual disappearance of the tumour, so as to render any opening of it unnecessary.

One of the results most dreaded in the earlier operations on a main artery for aneurism, was, that the supply of blood to the

distal part would be insufficient for nutrition, and that gangrene would ensue. This fear was soon found to be groundless. Prof. Scarpa has observed, "that the whole body may be regarded as an anastomosis of vessels, a vascular circle: and he contends that the remark is so true, that even an obliteration of the aorta itself, immediately below its arch, may take place without the general circulation of the blood in the body being stopped." A case is recorded by M. Paris, in which such a disease of the aorta existed, in the body of a woman. While she lived, the blood which was expelled from the heart, was transmitted into the trunk of the aorta, below the constriction: and it got there by passing through the subclavian, axillary, and cervical arteries, into the mammary, diaphragmatic, and epigastric arteries. From these latter arteries, the blood passed into the vessels of the thoracic and abdominal viscera, and those of the lower extremities (Cooper's Dict.). Other cases by Monro, Graham, and one by Dr. Goodson in Dublin Hospital Reports, vol. ii., p. 193,

1813, prove that the obliteration of the canal of the aorta at any one part, is not incompatible with the continuance of life; and further, they rendered unnecessary the experiments performed on dogs by Sir Astley Cooper, to prove, if the abdominal aorta is ligatured, the blood would be carried by the anastomoses to the posterior extremities.

It is not by experiments performed on healthy animals, that we can hope to gain sound knowledge in the treatment of diseases of the circulating system in man. Hunter's operation for aneurism was the result of reasoning and experience; disease had already proved, what Sir Astley Cooper sought to prove by experiments on dogs; and our knowledge gained by the practice of surgery, had proved that the lower limb could be adequately supplied with blood after ligation of its main artery. We read in the Report of the Royal Commission the following:

Q. 1049.—You have stated that you consider that experiments involving cruelty to animals have been too frequent, and that they

have not led to the mitigation of pain, generally speaking; but I presume you did not mean to say that they have not led to the successful treatment of complaints, or the mitigation of human suffering at all?

Answer by Sir William Fergusson:—

"With reference to that, I may, perhaps, speak more confidently regarding surgery than other departments in my own profession, and in surgery I am not aware of *any* of these experiments on the lower animals having led *to the mitigation of pain*, or to improvement as regards surgical detail."

In answer to further questioning the same great surgeon says: "Some of the most striking experiments that have been performed on the lower animals with reference to surgery have really been already performed, not experimentally, but on the best judgment, on the human subject."

"Now, John Hunter, who was one of our greatest physiologists, and allowed to be one of our greatest surgeons also, and who may be said to this day to stand at the head of what is

called Scientific surgery in this country, is specially celebrated for an operation which he devised on the arteries. That operation for sixty or eighty years stood as one of the most brilliant in surgery; and in so far as I have been able to make out (and I have inquired into the subject), Hunter's first experiment, if it might be so called, was done on the human subject, and it was long after he had repeated his operation on the human subject, and others had repeated it, that *the fashion* of tying arteries on the lower animals originated or was developed. The experiment in a surgical aspect might have been left entirely untouched, for Hunter had already experimented and developed the fact on the human subject."

Continuing our inquiry we shall endeavour to discuss fairly the asserted gains obtained by experimental research connected with the nervous system.

But before entering fully into the subject we will consider the often-quoted discoveries of Sir C. Bell.

Nor do we think we can do better than

refer to the writings of Sir C. Bell to glean how much of his discoveries was due to vivisection, and to learn his opinion of vivisection as a method of investigation.

He says, "The first conception which I entertained of the true arrangement of the nerves, arose from a comparison of the nerves which take their origin from the brain, with those which arise from the spinal marrow. The perfect regularity of the latter, contrasted with the very great irregularity of the former, naturally led to an inquiry into the cause of this difference. I said, if the endowment of a nerve depend on the relation of its roots to the columns of the spinal cord and base of the brain, then must the observation of their roots indicate to us their true distinctions and their different uses." p. 382.

"The spinal nerves are perfectly regular in origin and distribution. Each nerve has two distinct series of roots coming out in packets or *fasces*, one from the posterior column, and one from the anterior column, of the spinal marrow. The nerves thus formed

of two distinct fasciculi, are suited to perform all the offices of the trunk and limbs. Is it, then, by that combination of properties which they acquire through their double roots, that they are capable of performing their offices? And is this the cause of their simplicity of arrangement in their course through the body, as contrasted with the nerves of the head? Again, what cerebral nerves, in their distribution to the head and face, correspond in office with the spinal nerves? On the solution of these questions will depend our knowledge of the whole nervous system." p. 383.

"It was necessary to know, in the first place, whether the phenomena exhibited on injuring the separate roots of the spinal nerves corresponded with what was *suggested* by their anatomy. *After delaying long on account of the unpleasant nature of the operation*, I opened the spinal canal of a rabbit, and cut the posterior roots of the nerves of the lower extremities; the creature crawled, but I was deterred from repeating the experiment by the protracted cruelty of the dissection. I reflected, that an

experiment would be satisfactory, if done on an animal recently knocked down and insensible; that whilst I experimented on a living animal, there might be a trembling or action exerted in the muscles by touching a sensitive nerve, which motion it would be difficult to distinguish from that produced more immediately through the influence of the motor nerves. I therefore struck a rabbit behind the ear, so as to deprive it of sensibility by the concussion, and then exposed the spinal marrow. On irritating the posterior roots of the nerve, I could perceive no motion consequent, on any part of the muscular frame; but on irritating the anterior roots of the nerve, at each touch of the forceps there was a corresponding motion of the muscles to which the nerve was distributed. These experiments satisfied me that the different roots and different columns from whence those roots arose, were devoted to distinct offices, and that the notions drawn from the anatomy were correct."

"The key to the system will be found in the simple proposition, that each filament or

track of nervous matter has its peculiar endowment, independently of the others which are bound up along with it; and that it continues to have the same endowment throughout its whole length (for example, one of those in a compound nerve), and if its office be to convey sensation, that power shall belong to it in all its course wherever it can be traced: and wherever, in the whole course of that filament, whether it be in the foot, leg, thigh, spine, or brain, it may be bruised, or pricked, or injured in any way, sensation and not motion will result; and the perception arising from the impression will be referred to that part of the skin where the remote extremity of the filament is distributed."

"And so if we trace other filaments, whether they be for the purpose of sensation or of motion, each retains its office from one extremity to the other; nor is there any communication betwixt them, or any interchange of powers, further than that a minute filament may be found combined with filaments of a different kind, affording a new property to the

nerve thus constituted, that is to say, it accompanies it, and gives an additional power to the part where it is ultimately distributed." p. 377.

Brown-Séquard says, "The great theory of Sir Charles Bell was not that volition and sensation have their conductors in this or that place, but that these conductors are distinct one from the other all along from the brain to the periphery. It is this principle of a complete distinction between the elements of the nerves and of the spinal cord, which are employed in motion and in sensation, which is the great thing that science particularly owes to him."*

In pursuing his investigations, Sir Charles Bell was guided by anatomy, as he repeatedly informs us, and we find him evidently averse to *vivisection*, and only resorting to it to overthrow some pre-existing dogma, or to demonstrate his theories. Thus, in expounding his theory that the posterior roots were for sensation alone, it became as he thought necessary, to overthrow the opinion which prevailed, that

* "Lancet," July 3, 1858.

ganglia were intended to cut off sensation: or again, in demonstrating his idea of the analogy existing between the fifth nerve and the general system of spinal nerves, he made use of few experiments, and some of those were upon animals recently killed. "In fact, had Sir Charles Bell had recourse to experiments upon living animals, he probably would have succeeded in proving the exactitude of his theory concerning the roots of the spinal nerves." (Brown-Séquard).*

The same physiologist, speaking of a small pamphlet entitled "An idea of a new anatomy of the brain," says, "But however erroneous may be some of these views, we look upon this first work of Sir Charles Bell's on the nervous system, as an admirable production of the genius of this great physiologist. *The idea* of the distinction between the nervous elements employed in the different functions of the nervous system is there clearly and forcibly presented, and we may safely state that the *greatest part* of the recent progress of our

* "Lancet," July 3, 1858.

knowledge of the nervous system, both in a *practical* and in a *scientific* point of view, has its source in *this idea.*" *

This opinion of the theories of Sir Charles Bell, expressed by one of the greatest living physiologists, will afford little encouragement to vivisection; while the words of Sir Charles Bell himself, that "experiments upon the lower animals have never been the means of *discovery*," must convey condemnation to those who quote him as an authority for vivisection.

We are surprised that so many persons should be found to speak of Sir Charles Bell's *discoveries* as the *results* of vivisection: the so-named discoveries present to our view rather the outcomings of the working of a philosophic mind upon a well-stored collection of anatomical facts. The error arising from his theory of the cause of tic-doloreux, was corrected by the operations upon persons afflicted by this disease, proving failures, and assuredly no additional *experiments* upon animals were required

* "Lancet," July 3, 1858.

to ascertain a fact already demonstrated on the human subject.

The difficulties which beset us in any investigation to elucidate the functions of the hemispheres of the brain, may be recognised in cases of disease, where we have deviations the most abnormal presented in the functional activity of the organ, which leave no trace discoverable by our most advanced modes of investigation.

"In relation to common sensation and the effort of the will, the impressions to and from the hemispheres of the brain are carried across the middle line; so that in destruction or compression of either hemisphere, whatever effects are produced in loss of sensation or voluntary motion, are observed on the side of the body opposite to that on which the brain is injured." * This has existed almost as an axiom since the time of Aretæus, but now Brown-Séquard contests it, and brings forward two hundred cases of paralysis existing on the same side as the lesion.

* Kirkes' Phys. 534.

The investigations concerning, and the results obtained by, Galvanic or Faradic stimulation of the surface of the hemispheres, have led to considerable differences of opinion among experimenters as to their real signification.

Fritsch, and Hitzig, and Ferrier regard them as the result of excitation of the grey matter of the cortex, while Dupuy, Carville, and Duret have attempted to prove that the actions exerted are due to diffusion, and conduction of the current to the motor ganglia and motor strands.

Burdon Sanderson has also shown that when the grey matter corresponding to the centres, defined by Hitzig and Ferrier is removed, yet the same actions are produced by exciting the medullary fibres which proceed from these to the corpus striatum.

Schiff regards the movements resulting from stimulation of the hemispheres as of a reflex nature, but denies the existence of centres which exert any direct influence on the muscles of animal life.*

* Carpenter.

Concerning the functions of the cerebellum, experimenters have arrived at the following:

Brown-Séquard and Wagner purely negative results. Schiff says, "the functions of this organ are still unknown." Foville holds that the cerebellum is the organ of muscular sense, of co-ordination. Brown-Séquard maintains that it is not. (Kirkes' Phys.)

"As regards the cerebellum, we find sensibility persisting in the celebrated case of *absence* of this organ, recorded by Combette. There was also conservation of sensibility in two other cases in which the cerebellum had been *totally destroyed* by suppuration; at least it is stated that there has been no paralysis and no other trouble in the functions of animal life. One of these cases having been considered *impossible* by Bouilland, who read a report about it, at the Académie de Médecine of Paris in 1834, Marc, the learned physician of Louis Phillippe, rose and said that it was possible, as he had seen an exactly similar case at the Charité." (Quoted by B.-Séquard.)

"We mention the above cases only because they offer instances of destruction of the whole of the cerebellum. Had we to give more proofs, we could relate a very large number of cases of alteration or destruction of either or both lateral lobes of the cerebellum or of its middle part, without loss of sensibility and, frequently, with hyperæsthesia, as after an injury to the posterior columns of the spinal cord." (B.-Séquard: "Lancet," August 28, 1858.)

"From the results of vivisections it appears that when one of the crura cerebri is cut across, the animal moves round and round, rotating around a vertical axis from the injured towards the sound side. Such movements, however, attend the sections of *other* parts than the crura cerebri: and as indications of the functions of these parts, the results of such experiments have been hitherto almost useless." Experiments to prove the functions of the medulla oblongata, have proved nothing more, than injury and disease have demonstrated. Numerous instances are

on record in which injury to the medulla has produced instant death; and it is through injury of it, that death is usually produced in fractures and diseases with sudden displacement of the upper cervical vertebræ: the respiratory movements cease.

Dr. Magendie mentions the case of "a girl who lived to the age of eleven years, with the use of her senses, and with voluntary motion, weak, it is true, but sufficient for her wants, and even for progression. After death, no cerebellum nor mesocephalon could be found." (Elliotson.)

Mr. Lawrence saw a child with no more encephalon than a bulb, which was a continuation for about an inch above the foramen occipitale from the chorda spinalis, and to which all the nerves inclusively from the fifth to the ninth pair were connected. Breathing and temperature were natural: normal functions were discharged, the child took food, and at first moved very briskly, and lived four days.*

* "Med. Chir. Trans.," vol. v. p. 166, *et seq.*

Proceeding with our inquiry we come to the spinal cord, and we will endeavour to point out that experiments upon animals have furnished us with no results of value, inasmuch as the disturbance caused by the experiments has been such as to vitiate the results obtained: and our knowledge of the molecular changes occurring during activity of the nervous system has never reached beyond speculation. We will first consider the spinal cord as a *conductor* of nerve force.

Brown-Séquard in his lectures upon the nervous system, says, "Experiments upon the medulla oblongata, to decide if the crossing of the conductors of sensitive impressions, coming from the trunk and limbs, has taken place before they reach this organ or not, *cannot give positive results*, because the reflex movements are so energetic after a section of a lateral half of this nervous centre, that it is very difficult to know the degree of sensibility. *But pathological facts observed in man will settle the question.*"

Again: "I must say that it is absolutely

impossible to know, *while* we make a section of parts of the spinal cord, what is the precise depth of the injury; it is mere guess work." (Lect. iv., note.)

As far as experiments go, it is very difficult to decide whether the decussation of the conductors of sensitive impressions is absolutely complete or not. Brown-Séquard asserts that it is actually so. Oré, Longet, and Schiff, maintain the opposite. Perhaps the discrepancies may have resulted from the use of different animals; they however remain, and so render the question undecided.

"The question relative to the place of passage in the spinal cord, of the impressions of temperature, and of some other kinds of impressions, cannot be solved by vivisections. But *pathological facts observed in man will teach us much more concerning all the sensitive impressions which are not purely painful, than experiments upon animals.*" (Brown-Séquard.)

The grey substance of the cord is held to be the channel through which sensory impressions are conducted to the brain.

What are the elements of this central grey matter?

"This difficult question has not yet received a positive solution; but it cannot be doubted, at least, that the transmission takes place both by cells, and nerve-fibres united together, and not by cells alone acting at a distance upon their neighbours. Most of the nerve-fibres of the roots of the spinal nerves after having reached the grey matter, attach themselves with the nerve cells, and, as has been well demonstrated by R. Wagner, Bidder, and several of his pupils, these cells communicate with others in such a way, that two kinds of transmission are possible, one across the cord, and another towards or from the encephalon. But besides the nerve cells, and their numerous fibrils of communication, there are, in the grey matter, several collections of longitudinal fibres, forming very minute white columns, surrounded by the grey substance. These white columns, first well described by Mr. Lockhart Clarke, and after him by Professor Schroeder van der Kolk, cannot be considered

as the only channels in the grey matter for the sensitive impressions, or for the orders of the Will to muscles. The number of fibres they contain is too small for their having alone these functions, but it is probable they participate in it. Are they employed for a peculiar kind of sensitive impressions, while the other impressions would be transmitted by nerve cells and their communications? *This is a question that experiments upon animals cannot solve.*" (Brown-Séquard.)

And, he continues, "It is by far very much more difficult to determine what are the parts of the spinal cord employed in voluntary movements, than to find out what are those through which the sensitive impressions pass. I have long been in doubt in this respect, and even now, after having carefully watched a great many animals, on the spinal cord of which certain alterations had been made, and after having read a great many pathological cases, I still hesitate as regards various points." (Lect. iv., 1858.)

Is there any decussation of the voluntary

motor conductors in the spinal cord? The celebrated experiments of Galen, seem to answer positively that there is no such decussation.

Sir Astley Cooper made a section of a lateral half of the spinal cord of a dog, and produced a loss of voluntary movements in the corresponding side of the body. Most of the living experimenters agree upon this fact, that such a section causes paralysis only on the side injured. "I (says Brown-Séquard) have ascertained, a great many times, that this is not perfectly right. There is always, even in mammals, after a transversal section of the whole of a lateral half of the spinal cord, at least some appearance of voluntary movements in the side of injury, and always also a diminution of voluntary movements in the opposite side."

"*Vivisections* show that there is but a slight decussation of the conductors of voluntary movements in the spinal cord in animals, while *pathological cases* seem to show that there is no decussation of these conductors in this organ in man.

"*Anatomy* teaches that the anterior roots send

a large part of their fibres transversely across the cord, so that many fibres of the anterior roots of the left side decussate with as many fibres of the anterior roots of the right side.

"It seems *extremely probable* that these fibres, or at least many of them, are employed for reflex movements.

"The teachings of experimentation and of pathology, are both opposed to our admitting, that these decussating fibres are all voluntary motor conductors." (Lect. iv. & v., 1858.)

Nor are we able, speaking at the present time, to say that experimental physiology upon animals, has yielded us any more satisfactory conclusion, any "scientific result" upon this disputed question.

We have next to consider the spinal cord as a nerve centre, and we may well combine with it the medulla oblongata, which forms the encephalic prolongation, and which in function differs only in the importance and extent of the actions that it governs.

It is by the "reflex power" of the spinal cord, and the medulla oblongata that exist "the

conditions requisite for the maintenance of the various muscular movements which are essential to the continuance of the Organic processes;" and, as Dr. Marshall Hall has pointed out, it especially governs the various orifices of ingress and egress. (Carpenter, p. 680.)

"The spinal system," as Dr. Marshall Hall remarks, "never sleeps; it is constantly in activity." But it is by its "reflex power" that its actions are so especially protective. The word *reflex*, or *reflection*, applied to certain nervous phenomena, was first used by Astruc (1743), who sought to explain the functions of the brain, and particularly the motor reactions which follow a sensory impression, by a sort of *reflection* of the latter striking against the *columns of the brain*, and being reflected like a luminous ray from a polished surface. The researches of Prochaska enabled him to indicate the principal seat, and the substance also, of the phenomena which then took the name of reflex; finally, the histological study of the nerve globule, and its relations with the elementary fibres, afforded an opportunity of

making a more exact account of the mode by which this reflection is made, though in regard to this latter point *most of the facts are even yet hypothetical.* (Küss. Phys. Lect.)

The power by which the medulla oblongata governs and combines the action of various muscles for the respiratory movements, is an instance of the power of reflexion, which it possesses in common with all nervous centres: and a complete stop is put to the respiratory movements by the destruction of the medulla, and so to the continuance of the circulation.

Injury and disease in men prove the same as experiments on animals.* Numerous instances are recorded in which injury to the human medulla oblongata has produced instantaneous death: and indeed, it is through injury of it, or of the part of the cord, connecting it with the origin of the phrenic nerve, that death is commonly produced in fractures and diseases

* "Where, however, as in the Frog, the respiratory activity of the skin is equal to or greater than that of the lungs, the removal of the Medulla Oblongata is not attended by fatal results, and M. Brown-Séquard has kept frogs thus mutilated alive for eight months." (Carpenter.)

with sudden displacement of the upper cervical vertebræ. (Kirkes.)

It is thus animals are killed by pithing. Livy informs us that, at the suggestion of Hasdrubal, in the battle in which he was slain, when the Carthaginian forces were routed, and their elephants became unmanageable, the drivers destroyed them in a moment by one blow of a hammer upon a knife fixed between the junction of the head and spine (Histor. l. xxvii. c. 49). "But, it would appear that *much* of the reflecting power of the medulla oblongata may be destroyed; and yet its power in the respiratory movements may remain. Thus, in patients completely affected with chloroform, the winking of the eyelids ceases, and irritation of the pharynx will not produce the usual movements of swallowing, or the closure of the glottis (so that blood may run quietly into the stomach, or even into the lungs); yet, with all this, they may breathe steadily, and show that the power of the medulla oblongata to combine in action all the nerves of the respiratory muscles is perfect." (Kirkes.)

For a long time, the only proofs of independent power of the spinal cord as a nerve centre, were derived from the lower animals, and these could not be reconciled with the results of disease or injury of the spinal cord in man. Division of the spinal cord in man, and hence by inference in the lower animals, reduces the parts below to a state of complete insensibility. But in division of the human spinal cord, the lower extremities fall into any position that their weight, and the resistance of surrounding objects combine to give them; if the body is irritated, they do not move towards the irritation; and if themselves are touched, the consequent movements are disorderly and purposeless.*

In a decapitated frog, the limbs remain in, or assume, a natural position, resume it when disturbed. Irritation of the feet will cause leaping; and when parts of the body are stimulated, the feet are moved to push away the cause. It was therefore held, that the spinal cord of inferior animals possessed an

endowment which man's did not possess. And it was only by the direction of the attention by Dr. Marshall Hall, to pathological observations, that light was thrown upon the subject, and the want of analogy explained. We shall speak more fully of the pathological observations in our discussion of the second proposition.

Passing to the Sympathetic Nervous System we find that the general processes which it appears to influence, are those of involuntary motion, secretion, and nutrition.

The influence of the sympathetic nerves on the bloodvessels through the muscular elements in the coats of the arteries is exercised by nerve branches, called from their distribution and function *vaso-motor* nerves. It is now generally agreed, that the principal *vaso-motor nerve centre*, with which all these nerves communicate, and by which their action is regulated, is situate in the medulla oblongata—or that the vaso-motor fibres, arising from this nerve-centre, pass down the spinal cord, and issuing by the anterior roots of the spinal

nerves, enter the various ganglia on the pre-vertebral cord of the sympathetic, and thence reach their destination, probably taking with them fibres which arise in the ganglia through which they pass. It is, of course, difficult to determine the relative share exercised by the true sympathetic, and the ordinary cerebro-spinal fibres in the contraction of bloodvessels, and in the general processes of nutrition and secretion, since both kinds of fibres appear to be distributed to most parts, and there seems to be no possibility of isolating them. (Kirkes.) In fact, we are compelled, by the different and often opposing theories, to conclude that we are still far from decided as to the nature of the vaso-motor phenomena.

And we find that the study of the passage of the vaso-motor nerves offers unexpected complications and difficulties, which it is not easy to remove by experiment, their course, according to Schiff, differing in animals of the same kind under different circumstances. (Küss.)

It is also difficult to give a satisfactory explanation of the *dilatation* observed in the

vessels of certain regions of the body, when the nerves supplying them are irritated. Claude Bernard was at first disposed to admit a direct dilating action of the nerves, and Schiff seems still to believe in the possibility of such an influence. It involves, however, the view, that muscular fibres can be caused to elongate by irritation of their nerves, which is contrary to all analogy. (Carpenter.)

Nor are we reassured of experimentation having produced any scientific result, with reference to secretion—and we should be surprised, after what has just been stated concerning the bloodvessels, and their control by the nervous system, if opinions the most contradictory were not entertained of the phenomena connected with secretion. "The exact mode in which the nervous system influences secretion must be still regarded as somewhat obscure. In part, it exerts its influence by increasing or diminishing the quantity of blood supplied to the secreting organ in virtue of the power which it exercises over the contractility of the smaller blood-

vessels." (Kirkes.) Secretion in glands usually coincides with a turgid state of the capillaries. When the motor nerve is stimulated in the case of the salivary glands the circulation is accelerated and secretion takes place: but when the sympathetic nerve is excited, the vessels contract, the passage of blood is retarded in the vessels, and yet the power of secretion continues. The properties of the fluids produced are, however, not the same. We have hitherto, says Bernard, been at a loss to explain the results of this experiment: and we ought not perhaps to place implicit reliance on the stimulus employed, viz., galvanism; for M. Dubois Raymond has proved that on electrifying the nerves, an electrotonic state is produced, which does not act upon the muscles, but upon the nerves which lie in the vicinity; they are liable, in fact, to receive a part of the superabundant electricity condensed in the neighbouring trunk, sufficient to create a participation in the effects. (Lecture, 1861.) And, he continues, in our opinion, all the experiments hitherto

made on secretion require a *complete revision;* galvanism having almost invariably been employed in these researches, the results obtained up to the present time can no longer be relied upon, and the opinions expressed in a former part of this course, will perhaps be contradicted by future experiments.

We have failed to discover, in our present knowledge of the nervous system, any result of painful experiments upon animals, which can be considered of "scientific value." We find ourselves plunged into, what some one has called, "L'abîme des incertitudes," when we try to reconcile the phenomena, which have been wrung from living animals, by experiments upon a complex system, which in no two species is found exactly to correspond. One of the few things upon which most experimental physiologists agree, is, that the nervous systems of the various species of animals, used for experiment, differ often in some material manner: while man differs from all: his nervous system being the most highly complex, the crowning point of the

gradually but perceptibly ascending scale of constructive perfection. And with the increased complexity of the nervous system we recognise the increased futility of trying to adapt experiments upon animals, to explain the functions of the nervous system in man. Nor does it appear to be recognised by experimenters upon the nervous system, that they break through the chain of natural phenomena the moment they injure any portion of that system; they seem to forget what we are willing to believe, and they are anxious to prove, that a part of this system orders and regulates its own nourishment and perfection, as an integral part of the body. We allude to the influence of the vaso-motor nerve system, which regulates the action of the blood-vessels of the whole body, and so the nourishment of each individual part.

Concerning the brain, and the mode in which it discharges its functions, we have no results from experimentation in spite of the hacking and hewing to which the brain and its case have been subjected. The one law which

was considered established,—the law of cross-paralysis in disease or injury of either hemisphere of the brain, has for the present been overthrown by Dr. Brown-Séquard, who brings forward cases bearing witness to the contrary, and we shall see how anatomical research will clear up a problem, which no amount of experimental enquiry could demonstrate.

Nor has the examination of the surface of the brain when subjected to galvanic or faradic stimulation yielded us any reliable information up to the present time.

While the production of anencephalous children; the pathological investigations of Hughlings Jackson and others; and the cases recorded demonstrating the power of the brain to recover from severe mutilation, have proved all, and more than all the results obtained by experiments upon living animals.

Proceeding to the study of the cerebellum we find no *help* from experiments upon animals, but receive only vague hypotheses—injury, disease, and what may be termed nature's mutilations affording us all the real knowledge

of the functions of this part, we may be said to possess, and this is small indeed.

The medulla oblongata and spinal cord appear at first sight to have proved fields of enquiry rich in products by the use of experiments on animals; when however we come to analyze the results obtained we are agreeably disappointed.

The medulla oblongata, like the spinal cord, may be considered as a conductor of nerve force, and also as a nerve centre: and it differs specially in the nature of the reflex movements to which it ministers.

As a conductor of impressions, the decussation of some of the fibres of the anterior pyramids of the medulla oblongata explains the phenomena of cross-paralysis: and we have quoted the opinion of Dr. Brown-Séquard, to the effect that "experiments *cannot give positive results*, to decide whether the crossing of the conductors of sensitive impressions has taken place before they reach this organ or not."

As a nervous centre, the medulla oblongata has been regarded by many as the seat of

vitality—this has arisen more especially from the power which is exercised by the medulla oblongata over the various respiratory movements, which has been proved by injury to the medulla in the human subject being followed by instantaneous death, &c. (p. 41). Deglutition again, is performed through the agency of the medulla oblongata, as proved by its existence in monstrosities possessing no encephalon, and by the arrest of the power of swallowing when the medulla oblongata is injured. And it is in the medulla oblongata we find the centre of the vaso-motor nervous system. This is proved by anatomy.

We have shown (pp. 22–28) the share which vivisections held in the investigations of Sir C. Bell, we have also quoted the opinion of Dr. Brown-Séquard, with reference to the theories of this investigator, and we can only reiterate the opinion expressed in a former part of this essay. We have examined the evidence adduced concerning the spinal cord as a conductor and a nerve centre. As a conductor of sensitive impressions; experiments have given

rise to the greatest discrepancies as to the more or less complete decussation of the conducting fibres: and in answer to the question, "what are the elements of the central grey matter which is *held to be* the channel through which sensory impressions are conveyed to the brain? Brown-Séquard says, "this difficult question has not yet received a positive solution;"—and after further analysis, he concludes, "this is a question that experiments upon animals cannot solve."

The same experimenter admits himself to be still more in the dark with reference to the parts of the spinal cord employed in voluntary movements: and leaves quite undecided the question of decussation of the motor-conductors in the spinal cord.

As a nerve centre; the principal feature presented by the spinal cord may be said, to be that of reflex action, and as we have stated (p. 43), experiments upon animals to demonstrate this power, only produced confusion when the phenomena were applied to man.

From this summary of the principal func-

tions of the medulla oblongata and of the spinal cord, and of the evidence resulting from vivisections concerning them, we think we are warranted in arriving at the conclusion that no "scientific result" has been obtained.

Turning our attention to the sympathetic nervous system, and its connections and functions, we find ourselves in a maze of difficulty and uncertainty. We are told of the paramount importance of the vaso-motor system in regulating circulation, secretion, nutrition, and involuntary motion, and we are informed that "the study of the passage of the vaso-motor nerves offers unexpected complications, and difficulties which it is not easy to remove by experiment," and, "that we are still far from decided as to the nature of the vaso-motor phenomena," and, again, "it is difficult to give a satisfactory explanation of the dilatation observed in the vessels of certain regions of the body, when the nerves supplying them are irritated."

With reference to secretion, we are told, after a course of lectures, accompanied by

all the *necessary* demonstrations upon living animals, that, "in the lecturer's opinion, all the experiments hitherto made on secretion, require *a complete revision*," that, "the results obtained up to the present time can no longer be relied on," and that, "the opinions expressed in a former part of this course, will, perhaps, be contradicted by future experiments."

And, indeed, we should be surprised to find any useful result accruing from a method of examination fraught with so many uncertainties. Where, would we ask, is the anatomist so thoroughly conversant with the arrangements, the functions, and the connections of every part of the body, that he can assert his power to isolate, to control, to change, these vital properties with reference to any one part? Our knowledge of the anatomy of the vaso-motor system as gleaned from Quain and Sharpey, and others, convinces us that the investigator who sets about the task of demonstrating the processes of action of this system, by experiments on living animals, commences

a task he can never accomplish. The origin of the vaso-motors is from the centres, which are found principally in the medulla oblongata, and from whence fibres pass centrifugally, in especially the antero-lateral columns of the spinal cord, to give off the filaments from the anterior roots of the spinal nerves, which, blending with filaments of the sympathetic, combine to form the vaso-motor system. Thus then in any experiment we have to deal with a nerve which by its composition implicates the two great nervous systems of the body; and further, when we consider, in addition to this blending of filaments of the two systems, to form the vaso-motors, the repeated inosculations and intermixings of the various plexuses of the sympathetic nerves and ganglia; we are compelled to arrive at the conclusion, that to obtain reliable data from such a complex series of organisms by experimentation, is beyond our idea of human power. Indeed, while we are writing these pages, information reaches us which more than ever convinces us of the futility of experimentation, and the

Babel-like confusion of the language of the experimenters.

We have alluded to the opinion of Claude Bernard, concerning the results obtained from experiments upon certain nerves with reference to secretion. And we now read in a notice of investigations recently prosecuted, and published in the "Journal of Physiology," by J. N. Langley,—"These results are exceedingly remarkable inasmuch as they are exactly the reverse of what has hitherto been held to be the truth." And when we read that "all the doctrines which are actually received upon the physiology of the brain" will be opposed and combated by no meaner antagonist than that prince of vivisectors and chief of experimental physiologists, Brown-Séquard, in a course of lectures on physiology at the Collége de France, we are confirmed in the opinion we seek to enforce.

To Brown-Séquard's first lecture in the course alluded to, we would refer not with unmitigated pleasure, but with unbounded rejoicing and heartfelt thankfulness, and why? No man living has had the experience of the

lecturer in the field of enquiry we are discussing, but we find him expressing his intention *not* to engage in experimentation like his predecessor, Claude Bernard, for he observes, "Experiments in this matter are very difficult, for they are under the dependence of *irritation*, and they often fail. It is my confidence in the truth, and the number of facts which I have collected, that will sustain me in the course I have traced out for myself." (Progrès Médical, quoted in "Medical Times.")

Are there not fallacies underlying such a method of interrogating nature, which of necessity vitiate the results? * * * We shall in discussing this question quote some of the most apparent fallacies in connection with this mode of enquiry, and these will be selected from the writings of men whose names bear with them the stamp of recognition as authorities upon the subject.

But before proceeding to relate the more general sources of fallacy underlying such a method of interrogating nature, we would ask the reader to bear in mind that we are more

especially concerned with experiments which are *painful* in their performance. This qualification of itself, at once suggests two grounds for fallacy in the prosecution of any such enquiry, viz.: the disturbance caused by the pain if sensation is allowed to exist; and the derangement of the whole system if sensation is overcome by the use of anæsthetics.

The nervous system, it has been observed, creates a harmony between the different parts of the living frame, establishes a permanent connection between them, and renders them mutually dependent upon each other.

Any irritation or derangement of the nervous system caused by pain, shock, or their concomitants, destroys that harmony, removes the different parts of the frame from their normal condition, and so renders insecure the basis of any observation made under such circumstances. To quote words from the evidence of a supporter of vivisection as given before the Royal Commissioners:—

"It is of the utmost importance in physiological experimentation, that as little pain and

as little disturbance of the function as possible should be occasioned to the creature;" "you must minimise the pain and minimise the disturbance;" or, "so many disturbing elements are brought into the case, that it is excessively difficult to interpret the experiment."

The arrangement of the nervous system is so intricate, the machinery is so complicated, nay more, the action of the nervous element upon itself is so incomprehensible, that he must be a bold man who claims to rule and direct it, or boasts that he can measure or grasp the limit of disturbance of any one part. Bernard tells us that "an operation performed on *a single point* of the nervous system gives rise (*in one instance*) to a general hyperæsthesia (or exalted sensation) of the *whole* apparatus." In truth, the influence of that part of the nervous system which regulates the nutrition and function of every portion of the body, must ever be a barrier to man's inquisitiveness, and will ever prevent him reducing to scientific data those impenetrable agencies which govern and maintain life. To under-

stand the laws which regulate the nervous system is beyond our power, how then are we to analyze our disturbance of them?

If, on the other hand, sensation is deadened or suspended by the use of anæsthetics, from the moment that the anæsthetic agent enters the system, the whole organisation is removed from its normal condition, and the data recorded during such a state cannot be deemed reliable, must lead to fallacies in building up any theory.

Nor is it fair or truthful to hold up anæsthetics as a gilded bait to lure on the unwary. To many the mention of chloroform, æther et alia, conveys the idea of abolition of suffering —but ask the multitude, and evidence will at once be forthcoming that these blessings are not unmitigated boons, nay, that they are up to certain points extreme torture, and any one who has administered chloroform frequently to dogs must have seen this latter statement borne out with reference to them.

Dr. Brunton states, "Chloroform is inadmissible (*i.e.* for experimentation), as its ad-

ministration generally seems to cause dogs more pain than the experiment itself, and rabbits are very easily killed by it."

Mr. Pritchard observes, "When chloroform is given, they (dogs) suddenly, as a rule, become unconscious, and we frequently find that it is impossible to revive them."

Mr. Erichsen gives the following opinion:

"Chloroform, however, does not remove the physical impression produced on the system by a severe mutilation: hence the influence of a serious and prolonged operation is still manifested in the production of shock, of collapse, of slow recovery, even though the patient has suffered no *actual pain*. Certain operations appear to exercise a peculiar depressing effect on the nervous system, even though no pain be experienced."

Again, what says Dr. Brunton of chloral?

"Chloral," we are told, "in small doses produces sleep, apparently a natural sleep; you can wake a person readily from such a sleep. If you give an animal a large dose, you produce a greater effect; the sleep is then so profound

that the animal will not awake. You may cut it into pieces, or do what you please with it, it will lie as if dead. It is an anæsthetic for *animals*, but you cannot use it safely as an anæsthetic for *human beings*, because these animals that have got so large a dose of it *do not recover*." "I would not venture to give to human beings the doses I give to animals."

On continuing our enquiry, we find the most contradictory opinions with reference to Woorara or Curara, another supposed anæsthetic, and further that another, Chinoline, "would be a most important substance if it did not excite severe nausea and vomiting." "It is a remarkable fact, that where we had the more highly developed brain it caused this action; but in the rabbit, the guinea-pig, the hedge-hog, the frog, and the fish it had the effect of an anæsthetic."

Of what use is it speaking of Curara as an anæsthetic (which many do) when we have such proofs to the contrary as the following:

Lionville gives a description of a case where an overdose had been given, and artificial respi-

ration had to be kept up till the patient recovered. He says, "The patient then related all he had felt, the preservation of his intellect, the annihilation of all power of movement, of which he gave a clear account, witnessing all that went on around him, without being able to take any part in it; the fears freely expressed by some young assistants present being by no means reassuring to him." (Bulletin Général Thérapeutic, 1865, p. 404.)

Bernard, in the Revue Scientifique, mentions that "We have the accounts of individuals who had been inoculated with Curara, but to a degree which had not stopped the motions of respiration, and consequently permitted the individuals to return to life, I mean to movement. These have been able to relate that during their paralysis they had nevertheless been fully conscious of their existence, and of all the impressions which excited their senses." Or, speaking of Morphia, Bernard tells us, Revue des Cours Scientifique, vol. vi., p. 446, "Morphia is not an anæsthetic (does not annul sensation), but a narcotic (stupé-

fiant). When it has taken effect on a dog, he does not seek to escape; he has lost the knowledge of where he is; he no longer notices his master. Nevertheless sensibility persists, for if we pinch the animal, he moves and cries. At the same time, you see that morphia plunges dogs into a state of immobility, which permits us to place them on an experimenting trough, without muzzling or tying them."

We now pass to a consideration of the more general sources of fallacy underlying such a method of investigation, and we shall use the language of well-known physiologists for our purpose rather than the expression of our own thoughts. Brown-Séquard, Bernard, Ferrier, Bowman, Carpenter, and other well-known names in connection with this branch of science will be brought to support our conclusions. Dr. Carpenter tells us, that, "in such investigations, no useful inference can be drawn from one or two experiments only; in order to avoid *all sources* of fallacy, a large number must be made; the points in which all agree must be separated from others in

which there is a variation of results; and it must then be inquired, to what the latter is due."

Dr. Reid, whose name is so often quoted as an authority upon physiological matters, especially on Respiration, remarks: "The experimental history of the par vagum furnishes an excellent illustration of the numerous difficulties with which the physiologist has to contend, from *the impossibility* of insulating any individual organ from its mutual actions and reactions, when he wishes to examine the order, and dependence of its phenomena." (Carpenter, p. 394.)

Or, taking another nerve which has ever been a favourite with experimentalists, we read, "but it must be remembered that it is difficult to irritate the cervical sympathetic without coincidentally exciting the vagus-depressor nerve of Cyon, the laryngeal nerve, and descendens noni, which may materially influence the result." (Carpenter, p. 931, s. 715.)

Again, "on such subjects as the functions

of the different parts of the encephalon (brain), I do not believe that experiment can give trustworthy results; since violence to one part cannot be put in practice without functional disturbance of the rest." (Carpenter.)

"Vivisections upon so complex an organ as the brain, are ill-calculated to lead to useful or satisfactory results." (Bowman.)

As proof of the foregoing we may quote, "Experiments on the brains of monkeys, with special reference to the localization of sensory centres in its convolutions," by Dr. Ferrier and others. The conclusions of Dr. Ferrier having been communicated, in the discussion which followed, Dr. Nairne pointed out that other observers had arrived at conclusions differing from those of Dr. Ferrier, and that the brain of a monkey could not be taken as exactly similar to that of a man; but Dr. Brunton thought that the mistake made by German and other investigators who differed from Dr. Ferrier was, that they took the brains of animals lower even than the monkey to correspond with that of man.

M. Dupuy had arrived at different results. He said that he had found that when the centres of motion on *one* side of the brain were removed, paralysis followed for a short time throughout the corresponding part of the body, but that when the centres were removed from *both* sides of the brain there was no paralysis at all.* Dr. Brown-Séquard dissents altogether from the conclusions that have been drawn on this subject.

In speaking of experimentation upon living animals and observations of pathological cases, Brown-Séquard remarks: "The danger of making use of one of these means exclusively, is very strikingly illustrated by the many errors, concerning the cerebellum, committed by experimental physiologists, who mistook the effects of certain circumstances of their experiments, for the results of injuries, or of the absence of the cerebellum. Had they taken the trouble of comparing the phenomena they saw, with those observed, by medical men in cases of disease of the cerebellum,

* "Lancet," No. 2712, p. 289.

they would not have introduced in science a number of hypotheses which impede its progress. It is by so doing that experimentalists have thrown discredit on the means of scientific inquiry, of which they have made so much *use* and *abuse*."

The same experimentalist writes, as follows:

"I have ascertained that after almost a complete transverse section of the spinal cord, leaving undivided only the posterior columns, the transmission of sensitive impressions from almost all the parts of the body behind the section does not take place. This experiment performed by Stilling almost exclusively upon frogs, led him to affirm that sensibility is then entirely lost in all the parts behind the section. Much more recently M. Schiff, repeating this experiment, found, on the *contrary*, that sensibility is *not* lost in the various parts behind the section. MM. Vulpian and Philipeaux, who made this experiment some time after Schiff, positively declare that sensibility is completely and definitely lost."

"Experiments upon the medulla oblongata, to decide if the crossing of the conductors of sensitive impressions, coming from the trunk and limbs, has taken place before they reach this organ or not, cannot give *positive* results, because the reflex movements are so energetic after a section of a lateral half of this nervous centre, that it is very difficult to know the degree of sensibility."

"There are some animals in which the decussation in the spinal cord is not so complete, and so immediate as it is in mammals: such are reptiles and birds. This is one of the causes of some mistakes recently made by an able experimenter, M. Chauveau, of Lyons. He operated upon pigeons, and found, that after a section of a lateral half of the spinal cord, sensibility seemed to be much diminished on the same side, and not at all on the opposite side. I have ascertained that the results of the experiments vary with the place of the section. If it be made just above the lumbar enlargement, where M. Chauveau makes it, the decussation having hardly begun below

this place, the results are as he says; but, if the section be made two inches higher, in the dorsal region, there is, as in mammals, though less marked, an increased sensibility in the posterior limb on the side of the section, and a *diminution* of sensibility in the opposite limb. The loss of sensation is never complete, showing that the decussation is not complete. The same results are obtained in reptiles." But if we are to believe experimenters, the sensory paths in the cord are different in cats to those found in the man, the dog, and the rabbit. (Carpenter, p. 665, note.)

A very remarkable result of section of one half of the spinal cord is that, besides the anæsthesia (or lost sensation) which is established on the opposite side of the body, there is produced a state of exalted sensibility or hyperæsthesia on the same side. This condition, made apparent by the cries of the animal on the slightest pricking or pinching of the skin, begins to appear a few hours after the operation, rapidly attains its full intensity, and continues to be well marked for from

seventeen to twenty-two days in dogs, and from twelve to sixteen days in cats: after which, according to Schiff it gradually decreases, until at length the sensibility falls below its normal acuteness. Brown-Séquard has, however, observed it to persist in guinea pigs, though not in a very high degree of intensity, for many months after the operation.

The cause for this phenomenon has not been determined. Brown-Séquard seems to regard it as having a peripheric origin: whilst Schiff considers it rather as the result of irritation (as from the inflammatory process) occurring at the seat of injury: the latter view is supported by the observation of Chauveau, that it will sometimes ensue in cases where the vertebral canal has merely been opened, apparently as a consequence of the exposure and pressure of the cord against the edges of the wound; and also that of Türck, that hyperæsthesia may sometimes be observed in frogs on the same side of the body *above* the section.

Quoting from the works of Longet, M. Brown-Séquard remarks:

"These contradictions are certainly sufficient to show the untenableness of the systematic views of Longet, and it might seem useless to speak any more of these views; but as they have for a long while been admitted as correct by almost every one in France and in England, we must say a few words on the causes of the errors committed by that able physiologist. There are two means of ascertaining by experiments the functions of a nerve or of a part of the nervous system. One of these means consists in exciting the part, and in finding out what action takes place in consequence of the excitation; and the other consists in a section or extirpation of the part, and in examining what are the actions then missing. Of these two means Longet has made use of the first one only, and he declares that it is impossible to employ the second to the spinal cord. We shall see on the contrary that the first one could not be employed alone with success; whilst the second may very easily be employed, and furnish positive and direct proofs." (Lancet, 1858.)

Longet declares that it is impossible to lay bare the spinal cord of a mammal without producing, at once, such a debility in the posterior limbs that they lose, more or less completely, both voluntary movements and sensibility. Of course, if mammals were always in this condition after the opening of the spinal canal, it would be quite impossible to perform any valuable experiment on the spinal cord to find out what are its parts employed in the two functions which are then lost.

But, says Brown-Séquard, Longet has been mistaken, for this reason, that he opened the spinal canal in a considerable portion of its length, and in so doing produced a state of exhaustion by a great loss of blood, and by the excess of pain; if these are avoided there is no diminution of sensibility, and no diminution in voluntary movements. While Chauveau asserts that hyperæsthesia may occur from pressure of the cord against the edges of the wound.

M. Claude Bernard tells us: "We may, I believe, take it for granted that not only

morbid, but also physiological predispositions exist in man, as well as in the lower animals; even in a perfect state of health *each individual* retains his own peculiar habit of body, and is, in consequence, more liable to certain accidents than his neighbour. The various animals which serve for our experiments are far from exhibiting the same phenomena under the influence of agents entirely similar in their nature."

"Not only do the various species of animals differ in this respect, but even individuals of the *same* species are so far from resembling each other, that they cannot be submitted to the same experiments. So exquisite is the nervous sensibility of dogs of the higher breeds, that the slightest operations bring on fever, and are attended with alarming symptoms; they cannot, therefore, be employed in researches connected with the gastric juice, the pancreatic secretion, etc., etc., in fact, all operations performed within the abdominal cavity are liable to superinduce peritonitis in these highly sensitive animals, and generally

prove fatal. In dogs of a more vulgar class, how different are the results of similar experiments! During the operation the animal hardly attempts to move, and scarcely seems to suffer; the appetite remains unimpaired, and the secretions normal, in short, the various functions of the economy pursue their normal course.

"In the horse these differences are, if possible, still more strongly marked. The characteristics of certain breeds are, in colloquial language, attributed to *blood;* it would be more correct to attribute them to *nerves;* an irritable, sensitive, and highly organised nervous system is, in fact, the essential difference which separates a race-horse from one of those diminutive half-wild ponies, which hilly countries so abundantly produce. Would not the results of the same experiments be entirely at variance in these different animals? and what comparison could we possibly establish between them? It is, therefore, indispensable whenever great powers of endurance are required for the purpose of scientific re-

search, to select an animal of the lower breed; if, on the contrary, sensitiveness and nervous irritability appear desirable, none but the nobler kinds will afford the requisite qualities. Experiments on recurrent sensibility, for instance, which in the greyhound and pointer are generally successful, if tried on a shepherd's dog would fail in almost every case. Cold-blooded animals stand, of course, in this respect, at the very bottom of the scale. It will, therefore, easily be conceived that a state which in certain animals would constitute actual disease, may be perfectly natural in others."

The various predispositions we find in animals not only modify the phenomena of experiments upon them, "but also render them liable to diseases entirely different, when suffering from causes entirely similar. Being about to perform certain experiments on animals kept fasting for a long space of time, I left some dogs without food for several days; but during the severe frosts, these animals died unexpectedly. In making the autopsy, we discovered pneumonia in one case, pleuritis in

another, and inflammation of the bowels in the two last. Thus, under conditions perfectly identical, these animals were affected with totally different diseases."

And he goes on to say, "all exact calculations of physiological phenomena, is at least premature: up to the present time it has been an easier matter to observe facts, than to calculate them. Not to multiply examples uselessly, let me simply add, that it is far preferable, in the present state of science, to observe the extreme limits of phenomena, and to infer from them by a kind of compensating calculation, their average intensity, admitting, as possible, every intermediate degree.

In a word, it is impossible for us, in the present state of science, to *pretend* to anything like a *rigorous precision* in study of vital phenomena. But perhaps the strongest condemnation of the system pursued by Bernard, is to be found in his own words (Lect. xx. 1861): "In our opinion, all the experiments hitherto made on secretion require a complete revision: and the opinions expressed in a former part of

this course will, perhaps, be contradicted by future experiments."

As one more evidence of fallacy likely to arise in experimental research, we would mention the very conflicting opinions which are held with reference to the Glycogenic function of the liver. We have Bernard and his supporters, on the one hand, affirming that normally the liver continually, or at intervals, is changing its glycogen into sugar, and discharging it into the hepatic veins; Pavy and his followers, on the other hand, asserting that during life, under ordinary circumstances, there is little or no difference in the amount of sugar contained in the portal and hepatic venous blood, and therefore no conversion of glycogen into sugar at all. The question resolves itself into whether the blood of the hepatic veins contains or does not contain in life more sugar than that of the portal vein. Bernard takes the affirmative, Pavy the negative.

Bernard tells us that the existence of sugar in the blood, is a physiological phenomena as constant and permanent in the organism, as all

the other phenomena of nutrition, of which, indeed, it is a direct expression: and that there exists in the living organism a glycogenic function which maintains and regulates the proportion of sugar. In the arterial system, the proportion is sensibly equal everywhere; but in the venous system it is variable, and always inferior to that of the arterial blood, the only exception being in the hepatic veins, the blood of which is richer in sugar. Pavy maintains that no material difference exists between the amount of sugar contained in arterial and in venous blood. He also holds that the blood in the hepatic veins, if care be taken to keep the animal in a perfectly normal condition, contains no more sugar than does the blood of the right auricle or of the portal vein.

Bernard tells us that an augmentation occurs every time the animal is bled; Pavy points out that the very rapid changes which take place in the blood under altered conditions of the system, render it essentially necessary that the greatest precaution should be observed in order to obtain blood in its natural condition.

If the blood be taken from the living animal the latter must be perfectly tranquil, as violent muscular exertion or embarrassment of the respiration will occasion a conversion in the liver of glycogen into sugar; and as one of the effects of anæsthetics on animals is to occasion an abnormal amount of sugar in the blood, to attain accuracy the blood must be taken when the animal is not under their influence. If taken from the dead animal it should be procured as instantaneously as possible after death, so as to be unaffected by the *post mortem* production of sugar that occurs in the liver.

"After all," says the writer in the British Medical Journal, from whom we quote, "even should we accept Dr. Pavy's figures, it does not follow, because his experiments indicate no material difference in the amount of sugar in venous and arterial blood, that no such difference exists. The difference might be so slight as to fall within the limits of the errors of experiment; and yet, when the rapidity of the circulation is taken into account, it is easy to see that over a comparatively short period

of time, great additions and subtractions might occur in the circulating fluid which were incapable of being recognised at any given moment by the means at our present disposal.

Professor Ferrier says, "We know, and are every day confronted with the fact, that the most widely abnormal deviations from healthy functional activity of the nerve centres may be manifested, which leave no trace discoverable by ordinary dissection, or even by *any* of our most advanced methods of investigation. Nor do the facts of experimental physiology seem so consistent with themselves, or with the *undoubted facts* of *clinical research*, as to inspire us with unhesitating confidence as to their accuracy, or as to their applicability to human pathology."

"If it is difficult to test the mental condition in a human being, how much more difficult must it be in the case of the lower animals? And yet, from the way in which some have treated this question, one would be led to believe that nothing was more simple.

Flourens' conclusions are, I think, answerable for many erroneous notions which have long dominated cerebral physiology and pathology. One great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals low in the scale, are at once capable of application to man without qualification: an assumption which vitiates the conclusions of numerous physiologists of the present day. The very fact that there exist such patent differences between the effects of destruction of the cerebral hemispheres in different orders of animals ought, one would think, to inspire caution in the application to man of the results obtained only by experiments on the brains of animals low down in the scale."

"A frog deprived of its cerebral hemispheres still remains capable of a number of the most complicated and adaptive reactions, so little differing from those normally manifested by this animal that, except for the defect of spontaneity, they might be regarded as identical. But no one will say that the symptoms pre-

sented by the brainless frog at all resemble the clinical picture of a case of disorganisation of the cerebral lobes in man. The same may be said of pigeons.

"Nor are the phenomena in the case of the much-experimented-on rabbit at all comparable to those observed in cerebral disease in man. We might be led, from the effects of ablation of the cerebral hemispheres in this animal, to regard the cerebral hemispheres as having special functional relation to the upper extremities, as these are more particularly paralysed; and this conclusion has a germ of truth in it, when looked at in the proper light, but is a grave error if applied, without qualification, to human physiology."

We read in Carpenter's Physiology (p. 651) concerning the ophthalmic branch of the fifth nerve, that, "when the whole nerve, or its anterior branch, is divided in the *rabbit*, the pupil is *exceedingly contracted*, and remains immovable; but in *dogs* and *pigeons* it is *dilated*."

Dr. Radcliffe Hall (Edinburgh Med. and

Surg. Jour. 1846-48) in speaking of the ganglion of the sympathetic connected with the fifth nerve, and known as the ophthalmic or ciliary, says, "in the rabbit, the iris receives fibres from the sixth pair which do not pass through the ganglion; and it is through this that the contraction of the pupil is produced in that animal by irritation of the fifth pair, which will not produce any effect upon the pupil of the dog, cat, or pigeon, so long as it does not affect the brain to the extent of producing vertigo, nor affect the visual sense in any other way." (Carpenter, p. 652, note.)

How, we would ask, are we to apply such a result to man? Shall we class him with the rabbit, or with the dog, the cat, and the pigeon? or if the recurrent laryngeal nerves are the subject of interest in order to understand the mechanism of nervous dyspnœa in aneurism, shall we class him with the cat, or with the dog and the rabbit. If the latter, irritation of the recurrent nerve might produce spasm of the glottis; but if the former,

the so-called spasmodic dyspnœa, when of purely nervous production, was really due to paralysis."*

Experiments pursued to discover the action of drugs upon animals, must be looked upon as a most unreliable basis on which to form an opinion of their action upon men.

The results yielded by such a course must convince the most sceptical that disappointment alone can accrue to the enquirer.

In the experiments, to investigate the action of Calomel as a chologogue, a dog's stomach was found "unfitted to aid the action of the drug, a thick viscid mucus held the medicine," it was therefore "introduced into the duodenum," a proceeding, to our mind, about as philosophic as would be a slightly reversed experiment, "an attempt on the part of the investigator to digest the ordinary meal of a dog." Or again, many vegetables are known to be entirely innocuous to some animals, which are poison to our species, *e. g.*, rabbits will consume Belladonna with impunity, *ergo*,

* Dr. Powell, "Brit. Med. Journal."

"let the investigator give it to his children." "A concise answer to the question, what is poison? really seems more difficult than ever. Men gradually habituate themselves to the use of opium, tobacco, &c. till their daily dose is sufficient to kill from two to ten of their own species. Sheep have been known to consume unwholesome plants until their flesh becomes uneatable. Goats will feed on hemlock; hedgehogs swallow almost anything; and the common toad cares little for hydrocyanic acid. Ultimately we come to the acari, which appear to enjoy a perfect immunity from the usual effects of a *so-called poison;* for here strychnine is only a poison in the same sense that starch would be poison to man, namely, in that it does not contain every element necessary for the reproduction of tissue." (Lancet, quoted in appendix of report.) "I have seen Dr. Richardson give a pigeon enough opium to kill a strong man, and yet the bird was in no way affected." (Dr. Thorowgood, Med. Times and Gazette, Oct. 5, 1872.)

How are we to account for the apparent

contradiction presented to us in the following. Small doses of arsenic continued for a long time, produce poisonous effects (Harley). Arsenic eating, which is stated to be carried on extensively in Styria, is not only innocuous but even protective—that in Styrian works the workmen take arsenic with a view of escaping the poisonous effects of the fumes.

The effects of jaborandi, codein, and the results of experiments to establish the antagonism between various therapeutic compounds, are so unsatisfactory that we may consider them useless as scientific acquirements, and we may well abandon them as anything but encouraging.

And to revert to the action of mercury as influencing the secretion of bile, experiments upon dogs produced anything but conclusive information, and the facts produced by the committee, were deemed unreliable by many. Nor can we do less than endorse the words of Prof. Gross of Philadelphia upon the subject, when he said, that, "an experiment on a dog was one thing, but a careful observation on

the human frame performed at the bedside was another and wholly different one."

The long-entertained ideas prevalent with reference to the action of mercury, established by repeated clinical observation of the effect on man, are not likely to be readily ignored by medical men, or to be abandoned because a dog's alimentary canal fails to be similarly affected. If medicines are to be used for man's advantage, let us investigate their actions in the most reliable manner, and this will assuredly be by observations upon man himself. Even this method will be found in very many instances difficult enough from the varied idiosyncrasies of the race, but it will be devoid of the perplexities caused in having to decide man's greatest resemblance in some particular to the dog, the cat, the rabbit, or the pigeon respectively.

We have selected the foregoing remarks upon the probability of "fallacies underlying such a method of interrogating nature," from the lectures of some of the greatest physiologists; they convey the opinions of men who

have devoted their lives to physiological investigation by vivisections combined with pathological observations, and they bring with them the weight of experience and mature thought. Such testimony, we think, must bring the conviction to an unbiassed mind, that experiments performed under such probabilities of fallacy, can never be deemed reliable, that they are uncertain and vitiated. Did we deem it necessary to pursue further the question of "fallacy underlying such a method of investigation," we might fill page after page with records of experiments, their contradictions, and refutations.

Take, for instance, the accounts of the influence exercised over the stomach by the par vagum:

Le Gaulois says, the removal of a piece of the nerve, or the destruction of that part of the brain with which it is connected, puts a stop, not to the muscular action, or to its circulation, but to the secretion of gastric juice, and to digestion.

MM. Leurel and Lavaique declare, that after

division of the nerve, and even the removal of six inches of each nerve, digestion proceeds as before, the only effect being paralysis of the cardia.

Sir B. Brodie and Dr. Magendie found digestion uninfluenced if the division was made, not in the neck, but close to the stomach.

Dr. Magendie asserts that division *did not* prevent vomiting. Dr. Haighton found that it *did.*

Longet and Bischoff have shown that these different results depended on whether the stomach were digesting or not.

Were we to examine the results of experiments in the investigation of the influence of the nerves acting upon the heart, we should find quite as wide discrepancies, and why? "It is difficult (? impossible) to irritate the cervical sympathetic without coincidently exciting the vagus depressor nerve of Cyon, the laryngeal nerve, and descendens noni." And, who will be bold enough to say that he can surmount this difficulty, that he can effectually isolate any given nerve?

Could such discoveries not have been arrived at by a broad and comprehensive study of natural phenomena, or those quasi-*natural facts which are the continual accompaniments of civilisation?*

We believe, and we shall endeavour to prove that it is *only* by a comprehensive study of natural phenomena that we can hope to arrive at scientific results in the prosecution of physiology. We do not deny nor question the *effects* produced by experiments on living animals, but we object entirely to such *effects* being considered *proofs* of what would take place under a natural state of things. Nor do we think our objection requires any stronger evidence to support it, than is to be found in the contradictory results obtained from experiments performed by men equally earnest in their enquiries, equally anxious to labour for truth, although they may have mistaken the road.

On the other hand, if we turn our attention to the phenomena existing in the vast chain of nature composed of links formed by the various organisms to be found in the

vegetable and animal kingdoms, and compare them with the phenomena presented to our view in the human frame by accident and disease, we shall find how much more satisfactory are the results obtained. Our means of examining, and analysing phenomena, increase with every advance in mechanical and scientific knowledge. And every day we have to allow that secrets of nature are laid bare and revealed, which had hitherto baffled enquiry, and had been deemed undemonstrable.

Take as one instance, the much-discussed and experimented-on question as to "whether more than a fixed quantity of blood can find its way into the brain?" Monro, Kellie, and Abercrombie advocated the theory that "the quantity of blood circulating within the cranium is always and necessarily the same." Dr. Burrows was the first to refute by experiment, an error which careful observation and the use of the ophthalmoscope would have refuted without any such proceeding.

Sir T. Watson (Pract. Physic, vol. i., p. 452) says, "I told you the other day, that the

contents of the living cranium were inscrutable by our bodily senses, could neither be seen by us, nor heard, nor handled.

"But in strictness of speech, this assertion has ceased to be absolutely exact.

"I must admit that a window has been found through which a small offset, or sample, as it were, of the brain may easily be inspected. Helmholtz's lately perfected instrument, the ophthalmoscope, enables us to survey by reflected light the whole interior of the eye, and to bring distinctly within our vision the beautiful arrangement, and the varying conditions, of its living inner structure. The retina may almost be regarded as a piece of the brain displayed at an aperture in its bony covering, or, at any rate, as an index or dial-plate upon which are inscribed tokens of some of its morbid states. It is fed by branches of the same bloodvessels, vessels that are furnished also with the same vaso-motor nerves; the character of its circulation, therefore, partakes of, and so far reveals, the character of the deeper circulation within the brain-case.

"Mr. Bowman has assured me that the ophthalmoscope has contributed nothing less to our knowledge of the diseased conditions of the eye, than the stethoscope has of those of the chest, and Dr. Hughlings Jackson makes a similar estimate of its comparative value for unravelling the complex symptoms of diseases of the brain."

Nor are pathological cases wanting to refute the theory we are discussing. Dr. Elliotson says, "I once enjoyed an opportunity of very distinctly observing the motions of the brain and making some experiments with respect to it.

"A young man eighteen years old, had five years previously fallen from an eminence and fractured the frontal bone on the left side of the coronal suture, since which time there had been an immense hiatus, covered by merely a soft cicatrix, and the common integuments. The hiatus formed a hollow, very deep during sleep, less so when he was awake; and varying according to the state of respiration, *i.e.*, very deep if he retained his breath: much more

shallow, and even converted into a swelling by a long continued expiration. At the bottom of the hollow I observed a pulsation synchronous with the pulsations of the arterial system, such as deceived Petrioli, Vandelli, and others, at one time the adversaries of Haller, who all foolishly confounded it with that other remarkable motion which depends upon respiration. I may add that this wound on the *left* side of the head had rendered the right arm and leg paralytic." (Physiology, p. 341, *note.*)

Dr. Kellie narrates the following case:—

"Mr. G., with a numerous train of distressing symptoms, which too well marked the existence of enlargement of the heart, and the violent propulsive energy of that viscus, had only one characteristic of any disturbance in the head. On looking upwards to the whitened ceiling of a room, he saw a darkened spectrum, which vanished and reappeared with great regularity. It was soon discovered that the appearance of this umbra was synchronous with the systole of the heart, so that he used

often in my presence to count his pulse with the utmost precision by keeping his eye fixed on the ceiling, and numbering every appearance of the spectrum. In this case it is presumable that by each contraction of the left ventricle of the heart, plethora of the cerebral blood vessels was produced, and therefore an excess of pressure upon the cerebral substance." (Sir T. Watson, vol. i. p. 328.)

The following may serve to prove the reverse, comparative emptiness of the vessels of the brain, and defect of requisite pressure:

"A gentleman, thirty years old, was reduced to a state of extreme weakness and emaciation by some complaint of his stomach. As the debility advanced, he became very deaf; and this symptom varied in the following instructive manner. He was very deaf while sitting erect or standing; but when he lay horizontally, with his head quite low, he could hear very well. If, when standing, he stooped forwards, so as to produce flushing of the face, his hearing was perfect; and upon raising himself again into the upright posture,

he continued to hear distinctly, as long as the flushing continued: as this went off, the deafness returned." (Abercrombie.)

And, to appeal to Nature, why the beautiful provision to prevent any overflow of blood to the brain in animals who procure their subsistence by grazing, in spite of their long necks, and those who, like very many aquatic birds, obtain their food by diving? The *rete mirabile* of the arterial system may surely be looked upon as one proof of an all-seeing Designer and Creator: the provision for a regular supply of blood to the brain, under the most opposite circumstances.

The brain was considered to be closely shut up in an unyielding case of bone. Its surface must therefore be exempt from the influence of atmospheric pressure.

Dr. Burrows says, in opposition to this doctrine, "The numerous fissures and foramina for the transmission of vessels and nerves through the bones of the cranium, appear to me to do away with the idea of the cranium being a perfect sphere, like a glass globe, to

which it has been compared by some authors. If there were not always an equilibrium of pressure on the parts within and without the cranium, very serious consequences would arise at the various foramina of the skull." (Sir T. Watson, vol. i. p. 326.)

Dr. Seiveking has also given an explanation of the anatomical and physiological arrangements of intercranial contents, which negatives the idea of their being removed from the influence of atmospheric pressure, and which provides for the varying quantity of blood (at different times) within the cranium, by the great elasticity of the medullary tissue, and by the arrangement of the ventricular and subarachnoid spaces, with their varying amount of contained serosity." (Aitken, Science and Pract. Med. vol. ii. p. 266.)

We have thus before us, anatomical and physiological reasons; the results of pathological observations; and the evidence of physical examination; from whence to derive our knowledge of this subject, in place of the numerous experiments on animals: we un-

hesitatingly elect the former as the more likely to yield reliable information.

The study of the functions of the brain and nervous system is beset with the greatest difficulties and perplexities. As we have shown, the dissimilarity existing between different animals, nay, between animals of the same species, and further between animals and man, render the application of the results obtained by experiments unreliable when used to explain disease in the human being. And, although the scalpel and saw of the inquisitor have been busily used to prove, apparently, how much could be done to the brain without annihilating its functions, accident has placed on record an instance which far outdoes any experiment we have read of, in the magnitude of its operation. We allude to the case, which is quoted by Brown-Séquard and Ferrier as the "American Crowbar Case." "A young man aged twenty-five, was engaged tamping a blasting charge in a rock, with a pointed iron bar, three feet seven inches in length, one inch and a quarter in diameter, and weighing

thirteen pounds and a quarter, when the charge suddenly exploded. The iron bar, propelled with its pointed end first, entered the left angle of the patient's jaw, and passed clean through the top of his head, near the sagittal suture in the frontal region, and was picked up at some distance "covered with blood and brains." The patient was for the moment stunned, but, within an hour after the accident, he was able to walk up a long flight of stairs and give the surgeon an intelligible account of the injury he had sustained—he ultimately recovered, and lived twelve years and a half afterwards." (Dr. Ferrier.)

We need not enter further into the detail of the case, and would only remark that the track of the bar, caused neither sensory nor motor paralysis, the psychological condition alone called for notice. "Cases of injury of one or other frontal lobe, without sensory or motor paralysis, are very numerous." Of cases of disease in these regions many are on record. In all these cases there was entire absence of sensory or motor paralysis; and in many there

was nothing recorded, or nothing calling for special attention as regards the mental condition." One good instance of arrested development of the frontal lobes without any objective symptoms as regards motility or sensibility, is described and figured by Cruveilhier. This was the case of a girl aged fifteen who had remained in a complete state of idiocy from birth. The pre-frontal regions or anterior two-thirds of the frontal lobes were completely wanting. But, indeed, the frequent association of idiocy with such defect of the frontal lobes is a generally recognized fact.

Why with these proofs before us shall we proceed to remove the pre-frontal lobes from monkeys? Is it at all to be expected that we shall gain much information from such a proceeding? The evidence at present is in the negative, even if man's brains and monkey's brains were as similar as some would make them out to be; a repetition by experiments upon monkeys, of what has already been demonstrated upon man would be useless.

We stated in a former page that the phe-

nomena of reflex action as obtained by experiments on animals were unexplainable when applied to man, until Dr. Marshall Hall pointed out the advantage to be derived from observation of the phenomena of disease and injury. The simple division of the spinal cord with the clean cutting knife of the experimenter, leaves the separated portion in a state of much more complete integrity than it ordinarily is after disease or injury. Much light has, however, been thrown on these phenomena of reflex action by the careful observation of cases of disease.

Dr. Budd has published a collection of cases, and we give the leading facts observed by him.

In a case of paraplegia, the result of angular distortion of the spine in the dorsal region, the sensibility of the lower extremities was extremely feeble, and the power of voluntary motion was almost entirely lost. "When, however, any part of the skin is pinched or pricked, the limb that is thus acted on jumps with great vivacity; the toes are retracted towards the instep, the foot is raised on the

heel, and the knee so flexed as to raise it from the bed; the limb is maintained in this state of tension for several seconds after the withdrawal of the stimulus, and then becomes suddenly relaxed." "In general, while one leg was convulsed, its fellow remained quiet, unless stimulus was applied to both at once." "In these instances the pricking and pinching were perceived by the patient; but *much more violent* contractions are excited by a stimulus, of *whose presence he is unconscious.* When a feather is passed lightly over the skin, in the hollow of the instep, as if to tickle, convulsions occur in the corresponding limb, much more vigorous than those induced by pinching and pricking; they succeed one another in a rapid series of jerks, which are repeated as long as the stimulus is maintained."

Giving other phenomena of reflex action, Dr. Budd adds, "In all these circumstances, the convulsions are perfectly involuntary; and he is unable, by any effort of the will, to control or moderate them." This patient,

subsequently, regained in a gradual manner, both the sensibility of the lower extremities, and voluntary power over them; and as voluntary power increased, the susceptibility to involuntary movements diminished, as did also their extent and power. In another case in which the paralysis was more extensive, the abolition of common sensation was not so complete as in the former instance; but of the peculiar kind of impression which was found most efficacious in exciting reflex movements, *no consciousness whatever was experienced.* Not less interesting was the circumstance, that convulsions could be readily excited by impressions on surfaces *above* the seat of injury: as by pulling the hair of the scalp, a sudden noise, &c. This proves two important points: first, that a lesion of the cord may be such as to intercept the transmission of voluntary influence, and yet may allow the transmission of that reflected from incident nerves: secondly, that all influences from impressions on incident nerves are diffused through the cord. It is further interesting to remark,

that the reflex actions were very feeble during the first seven days, in comparison with their subsequent energy. In another case of very similar character, it was three days after the accident before any reflex actions could be excited. It is evident, then, that the spinal cord must have been in a state of concussion, which prevented the manifestation of its peculiar functions, so long as this effect lasted; and it is easy, therefore, to believe, that a still more severe shock might permanently destroy its power, so as to prevent the exhibition of any of the phenomena of reflex action. (Carpenter.)

So many cases of this kind have now occurred, that it may be considered as a demonstrated fact that the spinal cord, or insulated portions of it, may serve in man as the centre of very energetic reflex actions, when the encephalic power which ordinarily operates through it is suspended or destroyed, and it is further evident that these movements are not more dependent upon *sensation* than they are upon the *will*, since they may be excited

without the consciousness of the individual, even when fully directed to the part.

Thus, in a case of paraplegia recorded by Hunter: when the patient was asked whether *he felt* the irritation by which the motions were excited, he significantly replied—glancing at his limbs,—"No, sir; but you see *my legs* do."

We said we should endeavour to prove by anatomical investigations the explanation of the cases quoted by Brown-Séquard in opposition to the received law of cross paralysis.

"Flechsig has investigated the course and relations of the several tracts of the brain and spinal cord, with special reference to their respective periods of development in the human fœtus, and to the course and limits of the secondary degeneration which occurs in consequence of cerebral and spinal lesions, according to the researches of Waller and Twick. This is a method which must be regarded as infinitely superior to mere anatomical or histological investigation of the healthy and completely developed cord.

"Flechsig shows that the pyramids or pyra-

midal strands are developed subsequently to the hemispheres, and from them, and are wanting in anencephalous fœtuses. Their connections can be traced above into the cortical regions, bounded by the fissure of Rowlands, and below with the postero-lateral, and partly with the internal aspect of anterior columns of the spinal cord. These pyramidal strands are subject to very considerable variations, in respect to their decussation at the anterior inferior parts of the medulla oblongata, and as to the relative proportion of fibres which proceed down the postero-lateral and antero-internal columns respectively. *As a rule*, the most of the fibres of the pyramid descend in the postero-lateral column on the opposite side, the rest on the antero-internal on the same side. But, occasionally, the rule is reversed, and in one case there was *no decussation at all.*

A similar case has recently been described by Pierret.

"The strands which are subject to this variation are those which, as we shall see, degenerate in consequence of lesion of the

motor centres, and the evidence is of the most satisfactory kind that they are the paths of *voluntary motor* impulses.* Dr. Ludwig Türck has shown that certain lesions of the *encephalon* produce a degeneration of nerve tissue in particular tracts, which may be traced continuously down the spinal cord, usually in the *anterior* column of the side affected, and in the *lateral* column of the opposite side; whilst, on the other hand, local lesions of the *spinal cord* as from caries of the vertebræ, or from the pressure of tumours, produce a like degeneration in certain tracts of the *posterior* columns, and sometimes also of the lateral columns, ascending towards the encephalon. Thus, it appears that the *posterior* fasciculi are liable to this secondary degeneration in the *centripetal* direction only: and the *anterior* in the *centrifugal* direction only: the degeneration taking place in each case, in the direction in which they ordinarily transmit nerve force. The mixed endowments of the *lateral* columns are also indicated by these phenomena." (Carpenter.)

It is only by the examination of transverse and longitudinal sections through the substance of the cord, such as those so successfully made by Mr. Lockhart Clarke, that we can obtain anything like a correct idea of the direction taken by the fibres of the roots of the spinal nerves within the cord. (Kirkes.)

Morgagni gives a case of cerebral lesion (separation of the corpus striatum from the cortex) which he had carefully observed and examined post-mortem. He was astonished to find paralysis apparently on the same side as the lesion; but, distrusting his recollection and the accuracy of his records, he asked of his students, on which side the paralysis had existed. "All in general and each one in particular answered without hesitation that it was the right side (the side of the disease); and for this reason," said he, "it is clear to me that *sometimes* the paralysis occurs on the same side as the lesion." (Ferrier.)

Dr. Brown-Séquard in giving his summary of conclusions concerning the functions of the spinal cord, says; "Pathological cases bear

out almost all the conclusions we have just related, and they are not in opposition to those few conclusions which they cannot prove. Experiments on animals *could not* lead to certain conclusions which I shall be able to draw from some of the pathological cases I intend relating." (*Lancet*, 1858.)

It would be quite impossible to give even a condensed account of these cases; they are recorded in the published works of this great physiologist, and are open to the criticism of all: we have read them and are satisfied. Section of the spinal cord to prove that reunion may take place, is to our mind an unnecessary proceeding; it has been demonstrated by injury that a nerve may be severed, and yet be reunited and perform its functions, and we may by analogy argue that the same tissues being required in both instances, there is the power on the part of nature to produce them. Sir James Paget quotes two cases of division of nerves; in one the wound was made with a circular saw through the radial and median nerves; in the other, the knife of a chaff-cutting

machine separated all the structures of the wrist but the ulnar vessels and the flexor carpi ulnaris muscle: in each case reunion ensued.

Perhaps the best case on record of the reunion and restoration of divided nerves is to be found in the British Medical Journal for August 10, 1878. There we find the following remarks taken from the address in surgery by C. G. Wheelhouse, F.R.C.S., senior surgeon to the Leeds infirmary.

In the *Lancet* of June 1st of the present year, a series of experiments are recorded as in progress in Germany, the object of which is to ascertain whether the nerves, like other structures, are not amenable to surgical treatment for their restoration after division and complete loss of function; but whilst the Germans are patiently experimenting to determine the point, it is my good fortune to be able to answer the question distinctly in the affirmative, as the following case will show.

On May 5th, 1875, a patient named Adam Smith, a labourer, aged 22, entered the infirmary at Leeds, under my care. He limped

into the ward on crutches, his left lower limb being completely paralysed and useless, and stated that he had come to request us to remove it as an incumbrance. Nine months previously he had fallen with the back of his thigh upon the sharp edge of a scythe. The wound slowly healed, leaving a cicatrix which measured nine inches in length, and during the whole time occupied in the healing, he noticed that the limb was slowly wasting and withering away, and that it manifested no sign whatever of any returning sensibility. Eventually when he was able to leave his bed, he found to his horror that, although the wound was healed, and the limb was apparently saved, it had become wholly useless to him. So far as the distribution of the sciatic nerve was concerned, the power of sensation was entirely lost; of voluntary muscular power he had none, and the joints were relaxed and flaccid.

"I need not detail to you all the efforts, vain efforts indeed, that he made to recover the lost powers of his limb; suffice it to say that, re-

ceiving no benefit from anything he did or tried, he finally came to the hospital to ask for its removal.

"The whole cause of the mischief was of course clear at a glance. The sciatic nerve had been divided, and in the healing of the wound, the separated ends had never been re-united. Here, if ever, was a fair opportunity to test the question: Are nerves which have been divided, and for long remained disunited, capable of restoration?

"After fully explaining to the patient the position of affairs, and obtaining his assent, I determined to make an attempt to re-unite the ends of the divided nerve.

"Having laid open the back of the thigh, and by a careful and deliberate dissection of the parts exposed the wounded nerve, I found it completely cut across; the two ends were firmly felted in cicatricial tissue two inches apart. On the upper one was a large bulbous swelling, the lower appeared atrophied, and somewhat wasted. Both were carefully loosened and detached; the bulb was removed from the

upper one, and each was then pared obliquely until apparently fresh nerve tissue was exposed. When I then attempted to bring them together, the nerve was found to be so much shortened, that I could not do so until I flexed the knee fully. This enabled me to make the ends of the nerve meet without strain; they were then carefully stitched together with very fine carbolized catgut thread, the wound was closed, the ankle was firmly lashed to the buttock, and in this position the patient was put to bed. Little by little, and in very wandering fashion, day by day, and week after week, sensation was found to be returning in the limb. At the end of five weeks I began slowly to relax the position, and let down the leg inch by inch, until at length it became straight again, and then to my intense satisfaction I found that the restored sensibility remained. By very slow degrees the power of voluntary motion also returned, and he has gone on improving. During the whole of the past winter he has worked in the fields as he was wont to do before his accident, requiring neither stick nor

support, nor help of any kind; and though the limb remains greatly inferior in size and nutrition to the opposite one, it is, to all intents and purposes, an useful member again."

An essay like the present will not admit of our entering into all or nearly all the supposed gains conferred by experiments upon living animals, although we could fain, if time and space permitted, discuss fully each and every one of them. We will, however, briefly refer to some of these, and we will select such as are most frequently quoted by the supporters of experiments on animals.

Transfusion is one of the supposed gains conferred on science by experiments upon animals. That many animals have been subjected to experiments for the purpose of trying this operation, no one who reads the history of transfusion can doubt. But we look upon transfusion more as an invention than as a discovery; and no amount of experiments upon animals could ensure its safety when practised upon man. Dr. Barnes writes, "To encourage transfusion, even in extreme col-

lapse, the experiments of Brown-Séquard may be cited. He made the following observations on *decapitated criminals*. Decapitation took place at 8 a.m. Eleven hours later all trace of irritability had disappeared. Injection was then made of blood drawn from the veins of the subject into the radial artery. It went in vermilion, and returned dark as in life. In ten minutes irritability had returned; movements in the muscles could be excited." The same writer quoting Hasse, says, "If human blood cannot be procured, a lamb may be used. This has been successfully done."

Although originally performed about the year 1668, or about forty years after the discovery of the circulation of the blood, the first case of human transfusion was fatal, and the results of some cases caused the Pope and the King of France to prohibit the practice. Now, thanks to experience gained, the unreasonable fears are removed, and the operation takes its place not as an experiment upon animals, but as a direct experiment on man.

The causes of the sounds of the heart have

been the subjects of much discussion; and with the result of producing most widely different opinions, "an extraordinary amount of discordance upon a matter of pure observation," especially with reference to the first sound.

The cause of the second sound has been decided by the very conclusive (?) proof, that by a certain interference with the normal condition of things the sound is not produced, the valve is pinned back, the sail is reefed to prove it will not flap. Of much greater use is the demonstration, given by Mr. Savory from experiments on the heart after death, to show the aid the valves of the aorta receive from the walls of the ventricles to support the column of blood.

Practically, we are told "that the second sound *may* come to acquire so completely the character of the first, that it is difficult to distinguish the two in any other way, than by the synchronousness of the first with the heart's stroke, and with the pulse in the arteries." (Carpenter.)

Dr. Latham, in speaking of the sounds of the heart, says, "It is not *all* physiology that can be made useful towards the knowledge and treatment of diseases, but only those parts of physiology which are undeniably true, and not only true, but easily and at once seen to be so. A great deal of what is termed physiology has turned out to be a mistake; and so far as it has got mixed up with our notions of disease (and this has happened to a deplorable extent), it has hindered the progress of practical medicine." (Lect. I., p. 9.)

"Clinical observations and pathological investigations in the post-mortem room, have done more to aid us in the advance of medical knowledge concerning the derangements of the heart's functions, the concomitants and the results, than all the experiments we have read of ever could. We learn the sounds of health, we appreciate the sounds of disease, these are the experiments which teach us."

We have entered fully into knowledge supposed to be derived from experiments with reference to the nervous system, because we

may safely call it the ruling power of the whole body.

Some may be prompted to inquire why we deal so sparingly with many of the so-called results of experiments on living animals: our answer will be that upon examination we do not find that they exist as such.

One of these will doubtless suggest itself to many, "the treatment of asphyxia," the recovery of those apparently dead by drowning: by very many claimed as a proof of the good results obtained by experiments on animals. Can they read the report of a committee formed years ago to inquire into the best method of treating such cases, observe the result of the investigations, and then claim anything to compensate for the seventy or more experiments on dogs? What is the result? For the benefit of those who may not have seen it, it reads as follows: "No definite conclusion concerning the relative value of the various methods of artificial respiration can be drawn from these experiments;" and the committee, therefore, "refer to the report of ex-

periments upon the dead *human body*." They will require another committee to decide between the conclusions arrived at, on that occasion, and the "ready method" recently introduced by Howard.

What scientific result is arrived at by the following evidence of drowning dogs? That one will recover after being deprived of air 3′ 50″, but is not likely to recover after 4′ 10″, and that puppies survive under such circumstances longer than mature dogs. Whereas, we have an authenticated case of a woman being submersed for fifteen minutes and yet she recovered.

We do not think it necessary to increase our number of the failures of *painful experiments* to elicit truth concerning this vexed question.

The system of vessels known as the lacteals or lymphatics, vessels which are intimately concerned in the absorption of the food and in the process of nutrition, may be said to be of no less importance than the circulating system of the blood. In short, our observations on

these vessels form the basis of all our knowledge of digestion and the assimilation of food.

Hippocrates knew that the nutritive portion of the contents of the alimentary canal was conveyed by certain vessels to the system. Erasistratus actually saw the lacteals containing chyle—ἀρτεριας, γαλακτος πληρεις.

In 1563 Eustachius discovered the thoracic duct, but he remained ignorant of its use.

In 1662 Asellius, an Italian anatomist, saw the lacteals by chance when demonstrating the recurrent nerves. He, however, did not trace the lacteals to the thoracic duct, and so to the left subclavian vein, but fancied they went to the liver, distributing the chyle through it for sanguification.

In 1649, Pecquét, a physician of Dieppe, was *removing the heart of a dog*, when he noticed a quantity of white fluid pouring from the upper cava mixed with blood. He at first thought he had opened some strange abscess; and, after pressing first on one part and then upon another, he compressed the mesentery,

whose lacteals were full of chyle, when instantly a large quantity of this poured from the superior cava. He traced the lacteals to the thoracic duct, and thus overthrew the doctrine of the liver being the great seat of hæmatosis.

Sir C. Bell says, "when the young anatomical student ties the mesenteric vessels of an animal recently killed, he finds the lacteals gradually swell; he finds them turgid if the animal has had a full meal, and time has been afforded for the chyle to descend into the small intestines; he finds them empty, or containing only a limpid fluid, if the animal has not had food. When he sees this he has had sufficient proof that these are the vessels for absorbing the nutritious fluids from the intestines. The actual demonstration of the absorbing mouths of the lacteal vessels is very difficult. The difficulty arises from these vessels being in general empty in the dead body, from the difficulty of injecting them from trunk to branch, in consequence of their valves; and, lastly, from their orifices never being patent,

except in a state of excitement. The anatomist must therefore watch his opportunity when a man has been suddenly cut off in health, and after a full meal. Then the villi of the inner coat may be seen tinged with chyle, and their structure may be examined." (Lect. p. 360.)

Lieberkühn and Cruikshank have been able to do this; the latter opened a woman who had died suddenly of convulsions after taking a hearty supper in perfect health. "Many of the villi," he says, "were so full of chyle that I saw nothing of the ramifications of the arteries and veins; the whole appeared as one white vesicle, without any red lines, pores, or orifices whatever. Others of the villi contained chyle, but in a small proportion, and the ramifications of the veins were numerous, and prevailed by their redness over the whiteness of the villi. In some hundred villi I saw a trunk of a lacteal forming a beginning by radiated branches. The orifices of these radii were very distinct on the surface of the villus, as well as the radii themselves seen through the external surface, passing into the trunk of the

lacteal: they were full of a white fluid. There was but one of these trunks on each villus. The orifices in the villi of the jejunum, as Dr. Hunter himself said, (when I asked him, as he viewed them in the microscope, how many he thought there might be) were about fifteen or twenty in each villus; and in some, I saw them still more numerous." (Elliotson, pp.123, 124.)

We would ask, why should the discovery or the demonstration of these vessels be claimed as a plea for vivisections; the words of Sir C. Bell are only confirmed by experience.

The use of anæsthetics, of chloroform and the like, is claimed as one of the gains resulting from experiments on living animals, but we at once assert that no basis exists upon which this claim can be founded.

Sir James Simpson brought forward quotations from Dioscorides, Pliny, and Apuleius, authors of the time of the Roman Empire, showing that in that age the root of the mandrake (atropa mandragora) steeped in wine, was given to cause insensibility (ποιεῖν

ἀναισθησιαν) in persons who were to be cut or cauterised; and that whilst the influence of this remedy lasted, a limb might be cut off without any pain or sensation.

The seeds of the rocket (eruca) infused in wine were taken, according to Pliny, by criminals about to undergo the lash, in order to induce a certain recklessness or hardihood of feeling.

The *bang*, or extract of Indian hemp, is now employed in India for the same purpose, and was used by the Chinese, Egyptians, and Scythians in very early times.

The modern history of anæsthetics dates back to the end of the last century, when the discoveries of Priestley, Black, and Cavendish created a new era in the chemical world, and gave rise to a new branch of therapeutics, called *pneumatic medicine*, whose votaries hoped to cure diseases, and especially consumption, by the inhalations of various kinds of gases. In following out this theory, Humphry, afterwards Sir Humphry Davy, was, about 1800, led to the conclusion that "nitrous oxide, (laughing

gas) appeared capable of destroying physical pain, so it might probably be used with advantage during surgical operations." This conclusion was arrived at by experiments upon himself and not upon animals. Not until the year 1844 was the use of this anæsthetic established, when a dentist, Horace Wells, acting upon Davy's suggestion, inhaled the nitrous oxide himself before one of his teeth was extracted, with the effect of producing a complete unconsciousness of pain; he also administered it to several patients with the same beneficial results.

Ether came next into use about 1846, travelling to us from America. "It was employed in every variety of surgical operation, from Cæsarian section, in which it was used by Mr. Skey, at St. Bartholomew's Hospital, on the 25th of January, 1847, down to tooth-drawing: it was used to tranquillise the insane, to detect feigned disease, and to diminish the sufferings incidental to parturition."

Ether, whose chemical symbol is $C_4 H_5 O$, is one of a numerous class of bodies, all com-

posed of hydrogen and carbon, with variable proportions of oxygen or some other electro negative. The late Sir James Simpson, believing that amongst these bodies some might be found superior to ether, made many experiments on himself and friends with chloride of hydro-carbon, acetone, nitrous ether, and other analogous substances, and at last, *on the 4th of November*, 1847, in company with Dr. Keith and Dr. Matthews Duncan, found in a heavyish liquid that had been put by and almost forgotten, an agent which was superior to ether in its narcotising virtues, and immeasurably more pleasant. This was *Chloroform.*

In the *summer* of 1847, Michael Cudmore Furnell, then a student of St. Bartholomew's Hospital, was residing in the house of Bell & Co. of Oxford Street, to perfect himself in practical pharmacy. Excited by the recent discovery of ether, he made many experiments on the different varieties of inhalers. One day, whilst looking for *sulphuric* ether, he found a dusty bottle labelled "*Chloric Ether.*" He boldly experimented on *himself*, and inhaled

some of this liquid, which produced a certain amount of insensibility without the suffocating irritation and choking caused by ether. He communicated his observation to Mr. Holmes Coote, who used the compound during several operations performed by Lawrence, in the summer of 1847.

But whilst Furnell, Lawrence, and Holmes Coote did not get beyond the observation of the fact, and did not know that chloric ether was an alcoholic solution of chloroform, Simpson not only discovered, but identified and investigated the properties of chloroform, published his discoveries and established its use. (Druitt's "Surgeon's Vade Mecum.")

Let no reader of these pages suppose that we would set aside the use of chloroform and anæsthetics in operations upon animals; it has often been a cause for regret on our part that these agents are not more generally used in veterinary surgery. We have only given this account to refute the assertion that "these blessings are due to experiments upon animals."

And, in conclusion, we would urge, that one

great means of advancing our knowledge of physiology and pathology, upon a firmer basis, would be the blending of the study of human with veterinary physiology and pathology. Why should there be the wide gap which now exists between the healers of men and the healers of the animals? The same Designer made both, and

> "while in your pride ye contemplate
> Your talents, power, or wisdom,"

say, where the insult to man's majesty, if he is placed in juxtaposition with animals by the student of nature's laws?

When men will be content to study nature as presented to us in the animal kingdom, and in man, with its laws of life and health, of disease and decadence, then only shall they make advance in the knowledge of those laws. But so long as they continue to interfere with and distort the workings of nature by their own devices and machinations, so long will be produced the contradictions we are obliged to hear from individual experimentalists.

Are such experiments morally justifiable?

"If all we find possessing earth, sea, air,
Reflect His attributes who placed them there,
Fulfil a purpose, and appear designed,
Proofs of the wisdom of the All-seeing mind,
'Tis clear the creature whom He chose t' invest
With kingship and dominion o'er the rest,
Received His nobler nature, and was made
Fit for the power with which he stands array'd,
That first or last, hereafter, if not here,
He too may make his Author's wisdom clear,
Praise Him on earth, or, obstinately dumb,
Suffer His justice in the world to come."

Cowper.

Man is ever ready to assert his kingship, his rule, his possession by God's gift of the animal creation: "All these I give unto thee," is very satisfactory, but, "All these are mine," follows. The animal creation must be looked upon as one of those talents which we use and render an account of, and we must use it to advantage, or we must suffer the sentence of the unprofitable servant. Possession brings responsibility; we claim the former, we must undertake the latter. We cannot have possession, without power over the thing possessed, and we are morally answerable for our trusteeship: "Moral precepts arise out of the nature of the case

itself, prior to external command." Pain is naturally abhorrent to our nature, and the infliction of it upon helpless animals is cruel and brutish: it tends to degrade us, and to obliterate the God-like charity we should aspire to follow, and of which we have an example in the dealings of our Saviour with man. The killing an animal outright for the use and sustenance of mankind, is, we believe, making use of our possession, in accordance with the designs of the Creator, if we are to learn from what we observe is the order of the universe; but the torturing an animal for one single moment, to say nothing of the more prolonged periods we read of, is inhuman, and deserves condemnation. The infliction of useless pain is antagonistic to the natural tendency of our faculties and feelings. Bishop Butler declares, that man, "from his make, constitution, and nature, is, in the strictest and most proper sense, a law to himself:" that, "moral precepts are precepts the reason of which we see." Melancthon says, "wherefore our decision is this, that those precepts which learned men

have committed to writing, translating them from the common sense and common feeling of human nature, are to be accounted as no less divine than those contained in the tables given to Moses; and that it could not be the intention of our Maker to supersede by a law given on a stone, that which is graven with his own finger on the table of the heart." (Analogy, p. ii. c. i.)

Our minds, however, naturally turn to Holy Writ, nor have we to read far before we find arguments against cruelty. Thus, in Gen., c. ix., vv. 3 and 4, we read, "every moving thing that liveth shall be meat for you; even as the green herb have I given you all things. But flesh with the life thereof, which is the blood thereof, shall ye not eat." And we are told that this law was given to prevent cruelty, the translation given by the best interpreters, being, "flesh or members torn from living animals, having the blood in them, thou shalt not eat." This law was given to Noah to prohibit the practice of eating living meat, or parts of animals while yet alive. Moses insists upon it through his law; and in I. Samuel,

xiv., vv. 32 and 33, we read that the people of Saul's army "took sheep, and oxen, and calves, and slew them on the ground, and did eat them with the blood." The aptitude of animals for food was declared to be their moving and having life, a danger appeared of misinterpretation, and that these creatures should be used living; a thing God by no means intended, and therefore this law. (D'Oyly and Mant.)

Surely, if this care for animals called for Divine law in those early days, we cannot suppose that the care and guardianship of the Creator is diminished in our time, and that our dealings with the beasts which are under "fear and dread of you," are likely to escape the All-seeing eye. The argument used by some, that the sanction of society is sufficient to justify any proceeding, is to our mind untenable; we see many things countenanced by society to which we should be afraid to append the title of "moral." Nor do we see any argument in the assertion, that we kill animals for pleasure, in sport, and to provide clothing

and ornaments, and, therefore, we may use animals for painful experiments. We cannot discuss the morality of sport in its various bearings; but, supposing it to be deserving of condemnation, is that a reason for holding it up as an excuse for greater evil? Because one or more evils exist, shall we increase the number, shall we maximise the intensity, and so increase our responsibility!

We are answerable for our actions, in virtue of our place in the scale of created beings, and let us look well to our position when called upon to account for the fulfilment of the duties of our calling. "When we arrive at a certain point," says Lord Bacon, "nature grows deaf, and answers no longer our inquiries." May we not apply this to the matter of our discussion? or in the words of Goethe,

> "How? when or whence? the gods yield no reply;
> Let *so it is* suffice, and cease to question Why?"

Abernethy, in speaking of experiments upon animals, observes, "He (the inquirer) ought further to consider the probable kind of replies that may be made to his inquiries, and the

inferences that he may be warranted in drawing from different responses, so as to be able to determine whether, by the commission of cruelty, he is likely to obtain adequate instruction. Indeed *before* we make experiments on sensitive beings, we ought further to consider, whether the information we seek may not be attainable by other means." (Physiolog., Lect. iv.)

And in the report of the commission appointed to inquire into the subject of experiments on animals, we find the following protest, quoted by Sir William Fergusson:—"We, the Court of Examiners for Scotland for the Royal College of Veterinary Surgeons, desire to express our opinion that the performance of operations on living animals is altogether unnecessary and useless for the purpose of causation. Signed, JAMES SYME, *Chairman.*"

If in the former pages of this paper we have brought forward evidence of the futility of such experiments, if we have adduced instances of the fallacy of classifying the effects of such experiments as facts, we have at least

done much to prove that such experiments are unjustifiable.

What can justify the mutilation of an animal to produce a state of system which is said never to occur spontaneously? Yet this is done often and again in the production of artificially produced diabetes.

What can justify painful experiments upon sentient creatures, which, read by the eyes of different observers, yield us the confused language we have quoted; which, in lieu of knowledge, give us the hybrid production of science run riot, to be found in the works of our experimentalists. Verily, the experimentalists must give us more than they can at present show before we can cede justification to those effects of their mutilations which they are pleased to classify as physiological facts.

Is not their tendency to harden the operator and blunt his moral sense?

"All the faculties," says Gall, "are good and necessary to human nature, such as it should be according to the laws of the Creator.

But I am convinced that too energetic an activity of certain faculties produces vicious inclinations, causes the primitive destination of propagation to degenerate into libertinism, the sentiment of property into an inclination for theft, circumspection into irresolution and a tendency to suicide, self-love into insolence, disobedience, &c." (Elliotson.)

When we read the evidence of Dr. Klein before the Royal Commission on the practice of subjecting live animals to experiments for scientific purposes, we cannot believe in the description he gives of his feelings concerning the sufferings of animals under experimentation. Rather would we compare him with a person who says, "I never took any pleasure in *moral ethics*, and would not give one penny for all the morality in the world." "Yet," as Dr. Tuton, Dean of Peterborough, remarks, "This gentleman wrote a book of about two hundred and fifty pages in defence of Christianity; and the volume is almost entirely confined to the internal evidences and moral excellence of the system. It is not unpleasant to observe

the natural feelings of people thus completely overthrowing their theoretical positions."

Let us quote the opinion of the Rev. Samuel Haughton, M.D., F.R.S., based upon "half a lifetime's experience of medical students in dissection rooms and vivisections." "My experience is, that the dissecting room degrades some characters, and elevates others; and knowing that it is a moral trial to any young man to pass through the ordeal of the hospital dead-house and the dissecting room, that it tries and tests his disposition, like the Lydian stone of the ancients, I would shrink with horror from accustoming large classes of young men to the sight of animals under vivisection. I believe that many of them would become cruel and hardened, and would go away and repeat these experiments recklessly, without forethought or foresight; science would gain nothing, and the world would have let loose upon it a set of young devils." (Report of Royal Commissioner.)

Abernethy, in his physiological lectures, gives his opinion upon vivisection. Lect. iv.,

"Mr. Hunter, whom I should not have believed to be very scrupulous about inflicting sufferings upon animals, nevertheless censures Spalanzani for the unmeaning repetition of similar experiments. Having resolved publicly to express my own opinion upon the subject, I choose the present opportunity to do it, because I believe Spalanzani to have been one of those who have tortured and destroyed animals in vain, &c., &c."

Again, "The design of experiments is to interrogate nature; and surely the inquirer ought to make himself acquainted with the language of nature, and take care to propose pertinent questions; but I know that these experiments tend to harden the feelings, which often lead to the inconsiderate performance of them. Surely we should endeavour to foster, and not stifle benevolence, the best sentiment of our nature, that which is productive of the greatest gratification both to its possessor and to others. I, at the same time, express an earnest hope that the character of an English surgeon may never be tarnished by the

commission of inconsiderate or unnecessary cruelty."

> "Trace Science, then, with Modesty thy guide:
> First strip off all her equipage of pride,
> Deduct what is but vanity of dress,
> Or learning's luxury, or idleness,
> Or tricks to show the stretch of human brain,
> Mere curious pleasure or ingenious pain."
>
> *Macilwain.*

It is difficult to imagine any man with a mind so callous, with feelings of humanity so perverted, as to allow of his inflicting the cruelty described in the following extracts from Dr. Elliotson's Physiology.

"Brunner, from one dog, removed the spleen at one time, and the pancreas at another, after which the poor animal *pancratice valebat*, but, to render it celebrated for experiments, he, on a third occasion, laid bare the intestines and wounded them for an inch and a half, sewed up the wound, made a suture in the abdominal parietes so badly that the intestines were found hanging out on the ground one morning, purple and cold, and then allowed the animal to lick the wound into healing. He also per-

formed the operation for aneurism in the artery of its hind leg, and paracentesis of its chest, injecting a quantity of milk into the pleura and pumping it out again. This even was not enough for the gentle Brunner; he gave the dog such a dose of opium, when it had recovered from the operation on the spleen, that it was seized with tetanus. But this also it got the better of, and lived upwards of three months with its master, "gratus mihi fuit hospes," after all these indulgences, and was at last lost in a crowd; stolen no doubt, because, "celebris ab experimentorum multitudinem,—vivum philosophiæ experimentalis exemplum, et splene mutilus variis cicatricibus notabilis."

Dr. Brachet says, "I inspired a dog with the greatest aversion for me by plaguing and inflicting some pain or other upon it, as often as I saw it; when this feeling was carried to its height, so that the animal became furious as soon as it saw or heard me, I put out its eyes. I could then appear before it without its manifesting any aversion. I spoke, and immediately

its barkings and furious movements proved the passion which animated it. I destroyed the drum of its ears, and disorganized the internal ear as much as I could; and when an intense inflammation which was excited had rendered him deaf, I filled up its ears with wax. He could no longer hear at all. Then I went to its side, spoke aloud and even caressed it, without its falling into a rage,—it seemed even sensible of my caresses." Nay, Dr. Brachet repeated the same experiment on another dog, and begs to assure us that the result was the same.

Dr. Magendie says, "*It is droll* to see animals skip and jump about of their own accord, after you have taken out all their brains a little before the optic tubercles;" and as to "newborn kittens," he says, "they tumble over in all directions, and walk so nimbly, if you cut out their hemispheres, that it is quite astonishing." (Journal de physiologie, t. iii. p. 155.)

The same experimenter, not satisfied with the operations of some of his predecessors in investigating the act of vomiting, found, that if the stomach is removed, and a pig's bladder

substituted, and connected with the œsophagus, the retching induced by injecting tartarized antimony into the veins, causes the diaphragm and abdominal muscles to compress it sufficiently to expel its contents into the mouth. (Mémoire sur Vomissement.)

But enough, and more than enough; the experience of ages tends to prove the modifying influence of familiarity, written in the old proverb, that "familiarity breeds contempt," seen in the student who witnesses his first operation on the human body with various degrees of heart-sickness, as compared with his more advanced and matured fellow-worker, felt in the breast of every parent who checks the hand of the young in their first prosecution of cruelty, lest the act should become a habit.

"Tu ne cede malis, sed contra audentior ito."

THE END.

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